"Let Knowledge grow from more to more."—Tennyson.

KNOWLEDGE

AN ILLUSTRATED

MAGAZINE OF SCIENCE

SIMPLY WORDED - EXACTLY DESCRIBED

KNOWLEDGE

An Illustrated

MAGAZINE OF SCIENCE.

SIMPLY WORDED—EXACTLY DESCRIBED.

Edited by A. COWPER RANYARD.

"Let Knowledge grow from more to more.
—TENNYSON.

VOLUME XV.

JANUARY TO DECEMBER 1892.

NEW SERIES, VOL. VII.

LONDON: WITHERBY & CO., 326, HIGH HOLBORN, W.C.

1892.

LONDON:
PRINTED BY WITHERBY & CO., 326, HIGH HOLBORN, W.C.

INDEX.

INDEX OF THE PRINCIPAL ILLUSTRATIONS.

KNOWLEDGE

AN ILLUSTRATED

MAGAZINE OF SCIENCE

SIMPLY WORDED—EXACTLY DESCRIBED

LONDON: FEBRUARY 1, 1892.

CONTENTS.

BRITISH MOSSES.

By the Rt. Hon. Lord Justice Fry, F.R.S., F.S.A., F.L.S.

(*Continued from page* 7.)

We have seen that Nature has been practising a piece of severe economy in doing without the spore and the whole machinery adapted for the production of spores and substituting a gemma for a spore. We shall now see her going a step further in the same course of economy and doing without the gemma. She will produce protonema from the existing Moss plant without the intervention of spore or gemma.

Fig. 15 represents a leaf of *Orthotrichum Lyelli*, a Moss

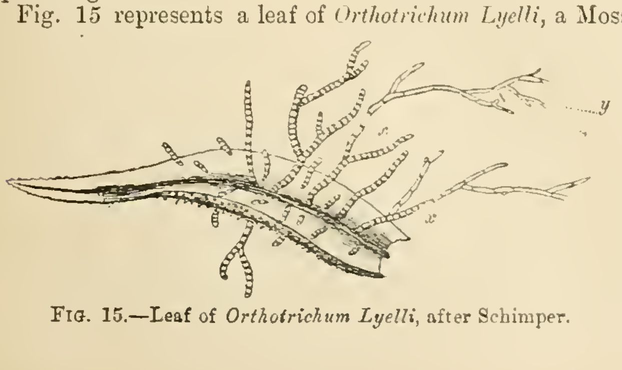


Fig. 15.—Leaf of *Orthotrichum Lyelli*, after Schimper.

found both in the old and the new world, on the trunks of trees: from this leaf, and especially from its midrib, are seen growths of protonema, and these gradually change into true roots, and on these roots buds are formed, which buds develop into true Moss plants. At *x* will be seen such a protonema divided into cells by transverse walls—further on the walls are oblique to the line of growth, and the growth then assumes the form of a root—and at *y* is seen a bud destined to produce a Moss plant.

Other species of Moss produce protonema from other parts of their structure; sometimes from the small roots or rhizoids, sometimes from the base or the margin of the leaf, sometimes from the stem, and sometimes from the calyptra or veil. These modes of reproduction are referred to in the third column in table B, under the heading "Moss."

But Nature has not yet tired of economy, she will try a yet shorter circuit of life; she will reject the protonema, as well as the spore, and produce on the existing Moss plant itself a bud which shall produce a new Moss plant. Sometimes leafy buds are formed on the true rhizoids, sometimes on the root-like hairs which hang suspended in the air and are known as aerial rhizoids; sometimes bulbs are found on the stem, and from these buds and bulbs fresh Moss plants arise. A yet more direct mode of propagation may exist, viz., the direct production of a Moss plant from a Moss plant; whether, in this case, a bud is first formed or not I do not know, though I should suspect the affirmative. This curious mode of reproduction is shown in Fig. 16, which depicts a plant of *Sphagnum cuspidatum*, on the ends of the loose leaves of which (*a*, *a*) are seen numerous young plants directly arising and alike in all things but size to the parent plant.

Fig. 16.—*Sphagnum cuspidatum*. *a a*, young plants at ends of branches. After Schimper.

The last-mentioned modes of reproduction are epitomized in the last two columns of table B.

There is one reflection which must almost have forced itself on every reader in considering this sketch of the development of Mosses, and of the economics of Nature in the process. The one object of her solicitude is the Moss plant—whatever else be left out, this is always present: Nature may strike off the spore, she may do without the gemma, she may avoid the protonema, but do whatever she may she

always produces the Moss plant, the vegetable growth with its stem and its leaves. To the production of this all else is subordinate; it is the one thing needful.

On the other hand, the one thing which Nature seems desirous to avoid is the sexual reproduction by the concurrence of the two organs of the archegones and the antherids. This is found only in the one mode of growth; every other kind of reproduction by gemma, by protonema, by bud, all of course leave out the whole sporophytic generation.

In the following table, which is far from exhaustive, I have endeavoured to exhibit some of the modes of reproduction, dividing them into those cases in which it takes place with protonema and those cases in which it takes place without.

Table C.—Modes of Reproduction.

	A.—With Protonema.	
i. Spores	in capsule.	
ii. Gemmæ	on end of leaf .. .	*Leptodontium gemmascens.* *Orthotrichum phyllanthum.* *Grimmia Hartmani.*
	on midrib	*Tortula papillosa.*
	in axils of leaves ...	*Bryum.*
	in balls	*Aulacomnion.*
	in cups	*Tetraphis.*
iii. Protonema ...	from zhizoids ..	*Phascum.* *Polytrichum.*
	from aerial rhizoids ..	*Dicranum undulatum.*
	from termiual leaves.	*Oncophorus glaucus.*
	from base of leaf ...	*Funaria hygrometrica.*
	from midrib	*Orthotrichum Lyelli.*
	from margin	*Buxbaumia aphylla.*
	from stems	*Dicranum undulatum.*
	from calyptra	*Conomitrium julianum.*
	B.—Without Protonema.	
iv. Leaf-Buds ...	on rhizoids	*Grimmia pulvinata.*
v. Leaf-Buds ...	on aerial rhizoids ...	*Dicranum undulatum.*
vi. Bulbs	on stem...	*Bryum annotinum.*
vii. Young Plants.	at ends of branches ...	*Sphagnum cuspidatum.*
viii. Leafy Branches.	becoming detached ...	*Conomitrium julianum.* *Cinclidotus aquaticus.*
ix. Rooting of maiu axis	... ...	*Mnium undulatum.*

Weismann's Theory.—The consideration of this table, and of the facts which are epitomized in it, is not without its interest in reference to Prof. Weismann's theory of the division of the cells and the plasma of organisms into two kinds: the germ cells and germ plasma endowed with a natural immortality, and the somatic cells and somatic plasma possessing no such endowment. That the Mosses are a difficulty in the acceptance of the theory as a universal truth, the professor himself admits. The evidence of the Mosses seems to amount at least to this: that in this whole group, the highest in this line of development, where the oophytic generation produces the principal plant, and where there are highly specialized organs for the production of spores or germ cells—that in this whole group either there is no effectual separation between the two kinds of plasma, or that the germ plasma is so widely diffused amongst the somatic plasma that every portion of the plant is capable of reproducing the entire organism.

Comparison with Zoological Embryology.—The table will further offer us some points of comparison with animal embryology.

In that branch of physiology, one of the most remarkable facts is what has been called recapitulation, *i.e.*, the summary in the life of the individual of the life of the race, so that the development of the individual tells the development of the race—*e.g.*, the gills of the tadpole tell us of the descent of the Batrachians from gill-breathing animals.

So here we cannot doubt that the protonema of the Moss tells us of the descent of the whole group of Mosses from the Algæ.

Another remarkable fact in animal embryology is the co-existence in exceptional cases of the mature and the immature form: so the axolotl retains both gills and lungs throughout its life. In like manner some Mosses retain their algoid protonema throughout life.

The Phascum or Clay Moss is a conspicuous instance of this curious fact: it is depicted in Fig. 17. It is a Moss of a not very high organization. The leaves grow close to the ground and the stem is very short. In like manner the sporangium (*a*) is almost sessile, and is seen almost enshrouded in the leaves, true rootlets or rhizoids (*r*) attach the plant to the ground, and the protonema (*p*) from which the plant has arisen survives and remains attached to it during the whole life of the plant. This protonema often exists in great quantity in the clay banks or fields where the Phascum dwells, and forms a sort of tangled mat.

Fig. 17.—*Phascum cuspidatum. a*, capsule; *r*, rhizoids; *p*, persistent protonema. After Schimper.

Again, in zoological embryology, an attempt is often found, to use the language of Prof. Milnes Marshall, "to escape from the necessity of recapitulating, and to substitute for the ancestral process a more direct method."

In like manner we have already seen to how great an extent Nature has adopted the system of short-circuiting in the reproduction of the Mosses; for in every mode of reproduction, except that through sporogone and spore, a shorter circuit is travelled. We have seen how in every case Nature seems to leave out the sexual reproduction if she can help it, and directs her whole attention to the production of the vegetative organism—the Moss plant in the popular sense—which she never omits.

Another point of comparison arises, but this time it is one of contrast between the embryology of the two kingdoms.

In animals, to again quote Prof. Milnes Marshall, "recapitulation is not seen in all forms of development, but only in sexual development, or at least only in development from the egg. In the several forms of asexual development of which budding is the most frequent and the most familiar, there is no repetition of ancestral phases, neither is there in cases of regeneration of lost parts."

In Mosses, on the contrary, the table last given shows that in most of the modes of reproduction the ancestral form, the algoid protonema, is retained and reproduced, whereas in the growth from a sexual cell, *i.e.* in the sporogone, the ancestral form entirely disappears.

Organization.—I now propose to describe somewhat more in detail certain parts of the structure of a Moss.

The *stem* of Mosses is, as we have already seen, very variable in size. Sometimes, as in the Phascum (Fig. 17), the whole plant is almost sessile; in other cases, as in the Polytrichum (Fig. 2), it attains to a very considerable length. In some Mosses inhabiting water, the length of the plant reaches to feet. In our flowering plants the stem is supported by the presence of fibro-vascular bundles, *i.e.*, fibres arranged in combination with tubes along which fluids can and do pass. But with the exception of one family, the stem of the Mosses, like all the other parts of the plant, is constituted of cells alone, and consequently the circulation of fluid in them appears to result entirely from the passage of fluid through the walls of the cells. Hence their close dependence on the presence of moisture; hence in dry weather they fade and droop, and with the return of moisture assume their wonted appearance.

The exception to which I have referred exists in the family of the Polytrichaceæ, of which the genus Polytrichum is the foremost (Figs. 2 and 3). In that kind of Moss the stem of the plant and the stalks that support the

capsules are of a firm, almost woody, structure, and give to the whole plant a different character to that of most of the Mosses. This peculiarity of the Polytrichum has, so to speak, enabled it to play a greater part in the world than most Mosses. Gilbert White tells us that the foresters of his neighbourhood made "neat little besoms from the stalks of the Polytrichum, common or great golden maiden hair, which they call silkwood, and find plenty in the bogs. When this Moss is well combed and dressed, and divested of its outer skin, it becomes of a beautiful bright chestnut colour, and being soft and pliant is very proper for the dusting of bed curtains, carpets, hangings, &c." But long before the dwellers in Wolmer Forest discovered this use for this Moss, it had been known to the pre-historic dwellers in our island, and had, it appears, been used by them to adorn themselves or their wives (themselves most likely). Curious fringe-like objects plaited of the stems of this Moss have been discovered in a crannog, or island fort, at Lochlee, in Ayrshire, attributed to that pre-historic period which has been called the late Celtic period. Furthermore, it is perhaps due to this fibrous character of the class that the earliest Moss of which we have any record in the strata of the earth appears to be one of the Polytrichaceæ.

The roots or rhizoids of the Mosses are distinguished by the minuteness of their growing ends, by their pliancy, and by the presence on their exteriors of a balsamic or glutinous deposit. To these points of structure they owe their capacity to insinuate themselves into the minutest crevices of rock, to get, for instance, amongst the particles of the oolites, and also to fix themselves in the shifting sands of the sea-coast, and by so fixing themselves to give fixity in return to the sand, and so tend to produce the sand-dunes in many parts of the coast. At some parts of the Northumbrian coast the *Racomitrium canescens* may be found buried deep in the sand, from which it can scarcely be detached; and in like manner the sand-dunes of Holland and the west of France have in many places been fixed by Mosses. The forests of firs on the North Sea and the Bay of Biscay thus owe their place of abode to humble Mosses.

Leaves.—When we examine the leaves of Mosses and compare them with the more familiar forms presented to us by the phanerogams, we find ourselves in a new world, and the interest with which we view them is increased when we remember that, according to the view usually accepted, they are, so to speak, a unique phenomenon; they are not the descendants of any earlier leaves nor the ancestors of any later ones; they appear thus once, as it were, in the history of the vegetable kingdom, and advance no further. They possess something of the charm which an ἅπαξ λεγόμενον exercises over the mind of a philologist.

We may first note what they are not. They are never opposite, never whorled, never on leaf-stalks, never truly veined, never lobed or compound, never furnished with epidermis or stomata.

When we turn to consider affirmatively what Moss leaves are, we find them in some cases characterized by an extreme simplicity of form. They are single plates of similar cells without midribs, without veins, and without border.

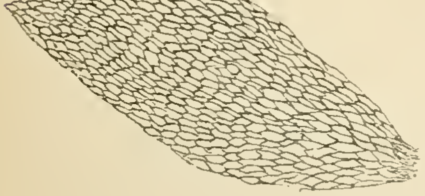


Fig. 18.—Leaf of *Hookeria lucens*, magnified, after nature.—A. F.

The accompanying Fig. 18, representing a leaf of the beautiful Moss, the Hookeria lucens, is an illustration of this form of leaf, and the Figs. 19 and 20 will show more highly magnified the structure of the component cells in a young and old leaf, and the grains of chlorophyll in the cells. In the old leaf a tendency will be observed in these grains to place themselves along the walls of the cells so as to produce the effect of thickened walls.

Fig. 19.—Cells of young leaf of *Hookeria lucens*, after nature.—A. F.

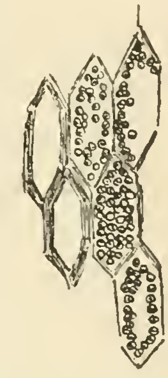


Fig. 20.—Cells of old leaf of *Hookeria lucens*, after nature.—A. F.

The leaves of Mosses stand in immediate connection with the atmosphere, absorbing moisture from it when moist, and shrinking and shrivelling when the air is dry. In some cases they are characterized by a marked difference in the form of the cells in the different parts of the leaf, and again in other cases by the unequal distribution of chlorophyll; in other cases we come across strange forms, the like of which we hardly know in the phanerogams; such are the thick border and double rows of teeth in some of the genus Mnium, the parallel plates in Polytrichum; and, stranger still, the third flange of the leaf in Fissidens, the true homology of which has proved a *crux* to bryologists.

A drawing of the leaf of *Fissidens adiantioides* is shown in Fig. 21—a thickened line of cells down the middle of the leaf assumes very much the appearance of a midrib, and on the right hand side occupying the lower half of the leaf is seen a third flange to the leaf, attached at its upper part to the leaf in an oblique line and after that to the vein or midrib of the leaf, so that in that part of the leaf there are, as it were, two sheets or plates instead of one. Various theories of the homology of this part of the leaf have been suggested. By some it has been thought to result from a vertical splitting of the leaf; but each of the two plates where they are doubled is of an equal thickness to the rest of the leaf. Some have suggested that the double portion is alone the true leaf and the rest an outgrowth, but this seems a violent assumption. Others, again, have suggested that the additional lobe is a stipule arising on the opposite side of the stem, which has become adnate with the leaf. Some of these suggestions carry conviction with them.

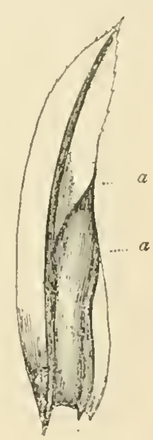


Fig. 21.—Leaf of *Fissidens adiantioides*, showing *a a*, the third flange. After Schimper.

In some cases the leaf is produced into a long thread or beak, devoid of chlorophyll, and often with indented or toothed edges. This structure is found chiefly in Mosses living on stones and rocks, and in dry situations, such as Grimmia and Racomitrium, and the presence of these long white threads or beaks gives a grey tint to the whole Moss, and in places where the Moss is predominant (as, for instance, some parts of Dartmoor and North Wales, where Racomitrium abounds) a grey tint to the whole landscape. These long hairs and prominences, especially when armed with lateral teeth, no doubt retain the moisture which is necessary not only for the vegetative life of the Moss, but also for the process of reproduction by archegones and

antherids; hence it probably is that this form of leaf prevails in Mosses living in dry situations, just as the thick leaves of succulent plants are found in similar situations.

The capsule.—Of all the organs of a Moss plant, the capsule which produces the spores is, perhaps, the most peculiar and characteristic. If the reader will refer back to Figs. 1 and 2, he will see the capsules (*c* in Fig. 1, *a* in Fig. 2) borne on the end of the long stalks (*s*). The capsule, as is shown in Fig. 2, is covered with a delicate veil or calyptra, which is shown as removed at *cal.* This veil is the remains of the archegone, borne up by the stalk or seta in its upward growth. In the case of the Polytrichum, it is covered with a thick coating of depending hairs, a circumstance which gives its name to the genus. When the veil is removed, the capsule itself is disclosed (*c* in Fig. 2), surmounted by an operculum or lid (*o*), which fits on to the top of the capsule like the lid on a box. The capsule with the lid removed is shown at the letter *c'*.

If the reader will refer back to table A, he will find that the Acrocarpous Mosses, *i.e.*, those which produce their capsules at the end of the axis, are divided into Stegocarpæ and Cleistocarpæ. We are now in a position to appreciate this distinction. In the Cleistocarpous Mosses, the capsule is never differentiated into the two parts of the true capsule and lid; it remains always as a closed capsule until the walls decay or break, and so emits the spores which it contains. Of this class, the clay Moss or Phascum (Fig. 17) is a familiar example. As a whole, this class is less highly organized than the Stegocarpous Mosses—such as the Polytrichum—where the capsule, originally a single organ, becomes differentiated into the two parts already described, and the spores are retained in the capsule dry and snug until the ripened lid falls off and allows their escape.

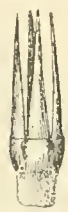


FIG. 22. Peristome of *Tetraphis pellucida*, after Schimper.

In some cases, the orifice of the capsule is formed by a smooth edge or lip; but in other cases this orifice is surrounded by a girdle of teeth of varying number, form, and colour, so that the study of the peristome, as this girdle is called, presents a continued variation of objects of beauty and interest. Fig. 22 exhibits the peristome of the beautiful little Moss the *Tetraphis pellucida*, to which I have already referred. No simpler form of peristome can be found than this, exhibiting four teeth in a single ring. Fig. 23 is part of the peristome of the *Fissidens adiantioides* magnified, and shows two phenomena common in peristomes, (1) the division of the teeth at their free ends, and (2) the presence of transverse markings, generally of a darker colour than the intervening spaces. A tooth thus marked is said to be trabeculated, *i.e.*, marked by trabeculæ, or little beams.

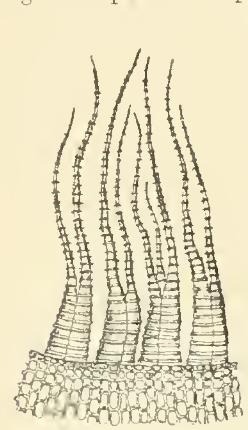


FIG. 23.—Part of peristome of *Fissidens adiantioides*, magnified. After Schimper.

FIG. 24. Peristome of *Tortula ruralis*, magnified. After Schimper.

In one considerable family of Mosses, some of which are very common on the tops of our walls, the teeth are hair-like in length and delicacy, and are twisted into a curious scroll like a lambent flame of fire. Fig. 24 represents one of these twisted peristomes from which the genus especially characterized by it has received from some botanists the name of Tortula.

Again, in another form, which exists in Polytrichum, the teeth assume a very different appearance and connection. To make this intelligible I must refer to a portion of the structure of the capsule to which I have not hitherto referred, the columella, or little column, a central stem which occupies the very axis of the capsule; this, in Polytrichum, emerges from the mouth and expands into a tympanum or drumhead, and the teeth arising from the lip of the mouth join and support this drumhead, leaving interspaces between them something like long narrow windows under the flat roof of a circular tower, through which the spores escape. Fig. 25 is a representation of this singular structure, in which *p.* marks the place of the peristome or girdle of teeth: these are seen to be attached to *t.*, the tympanum, into which the column has expanded.

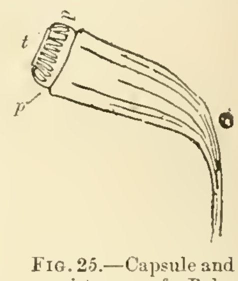


FIG. 25.—Capsule and peristome of *Polytrichum*. *p.* peristome, *t.* tympanum, after nature.—A.F.

I have hitherto spoken of the peristome as consisting of one girdle of teeth; often it is double as in the great genera Hypnum and Bryum, and then the teeth often reach the number of sixty-four. In one foreign genus (Dawsonia) there are as many as four circles of teeth.

The accompanying Fig. 26 is a diagram intended to assist the reader in gaining a general notion of the structure of the several parts of a capsule with a double peristome: it is a diagram only of a section of an ideal capsule, and not a picture or representation of any existing capsule. The reader who will carefully inspect it will learn what to look for when he first holds a capsule in his hands, and may get some assistance as regards the technical language of bryology. He will see the calyptra, or veil (*cal.*), the remains of the original archegone; he will see the operculum, or lid (*oper*),

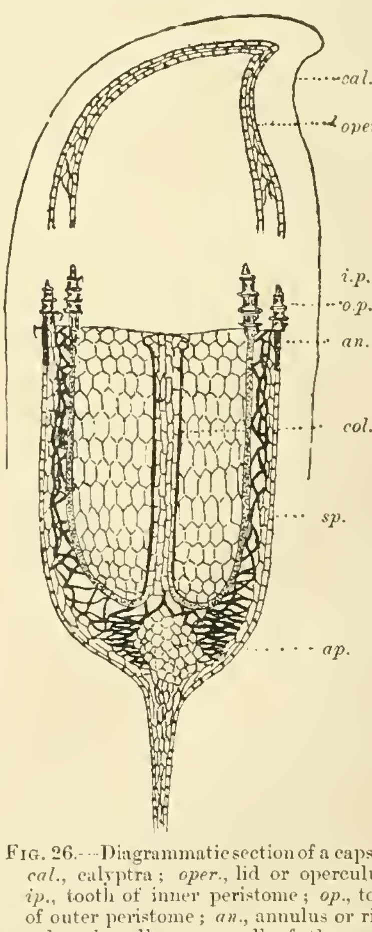


FIG. 26.—Diagrammatic section of a capsule. *cal.*, calyptra; *oper.*, lid or operculum; *ip.*, tooth of inner peristome; *op.*, tooth of outer peristome; *an.*, annulus or ring; *col.*, columella; *sp.*, wall of the actual spore sac; *ap.*, region of the aphyse.—A. F.

severed from the capsule itself; he will observe the double peristome (*i.p.* and *o.p.*), the outer teeth consisting of a prolongation of the outer coat of the capsule, the inner teeth arising in like manner from the wall of the inner sack or spore case, or sporangium (*sp.*); he will observe an interspace between these two sacks filled with cellular tissue; he will observe in the interior of the sporangium the cells which become spores with the maturity of the growth; and in the middle of the diagram he will notice the columella or column. At the base of the capsule he will see the region (*ap.*) which, when swollen or enlarged, gives rise to the apophyse. All these parts are subject to a great range of variation, but this diagram may nevertheless, I hope, prove of some assistance to those beginning the study.

The object served by the complicated structure of the peristome is not, perhaps, very certain, but it seems to be intended to secure the retention or exclusion of the spores from the spore sac in such conditions of the atmosphere as will best conduce to their germination. In the Gymnostomous Mosses (*i.e.*, those without peristome) it is observed that the spores sometimes germinate within the capsule, an event which is probably adverse to the prospects of the race. The following table will illustrate, in a few cases selected as illustrations, the different behaviour of the teeth of the peristome under different hygrometric conditions, and suggests what is the probable advantage in each case :—

TABLE D.

Genus.	Condition of teeth		Reason suggested.
	in dry weather.	in wet weather.	
Bartramia ...	Erect ...	Convergent	That spores require *dry* weather when first emitted.
Orthotrichum	Erect or reflexed ...	Ditto ...	Ditto
Funaria ...	Reflexed ...	Ditto ...	Ditto
Bryum ...	Convergent...	Expanded	That spores require *wet* weather when first emitted.
Fissidens ...	Ditto ...	Ditto ...	Ditto

The motion of the teeth of the peristome appears to be due to the action of the annulus, a ring of specialized cells which surrounds the mouth of the capsule at the base of the teeth: and the opposite ways in which these cells act in the same condition of moisture in different genera is a remarkable circumstance.

To anyone who studies the subject, the immense variety as well as beauty of the peristomes of Mosses becomes very impressive. If the sole end be the protection and extrusion of the spores in the proper weather respectively, why is there this infinite wealth and variety of form and colour? The question can be asked, but hardly can be answered: and the mind of the beholder is left, as it so often is when contemplating the richness of Nature, in a state of admiration and wonder and ignorance. "Rerum natura tota est nusquam magis quam in minimis."

(*To be continued.*)

THE CHEMICAL ELEMENT CARBON.

By Vaughan Cornish, B.Sc., F.C.S.

The older conception of a chemical element was that of a property, or plexus of properties, which being common to several substances was regarded as a *principle* existing in all those substances. In some of these old *principles* we can recognize the present chemical elements. But whereas we now think of the elements as the undecomposed residues of natural substances and as being forms of ponderable matter essentially uncreatable and indestructible, the earlier conception of a chemical principle was that of a constant *property* possessed by certain forms of matter, *e.g.*, the combustibility which is a property common to all substances of animal and vegetable growth. It is the dependence of certain properties on the presence of certain kinds of material substance which aids us in many cases to connect the older chemistry with that of the present time, by identifying some of the so-called *principles* of the seventeenth century with the corresponding *elements* of the nineteenth century chemists.

A short history of the study of Carbon will serve to show the gradual evolution of certain important modern ideas with regard to chemical elements.

When vegetable or animal materials are heated with a limited access of air the result is that they are charred. Materials differing in almost every respect, except in their being formed in the processes of animal or plant life, agree in this—that when treated as above described they yield a *char*. The word *carbo*, formed from the same root, appears to have been used in the Augustan age in the same sense, namely, to designate the char left by the partial burning of animal and vegetable bodies. It was usually applied to the char obtained from wood or wood charcoal. In the more modern use of the Latin tongue *carbo* generally means coal, the vegetable origin of which is readily recognized.

The first important generalization in the history of chemistry is that contained in the theory of Phlogiston, which retained its hold upon the minds of chemists till the later years of the eighteenth century. The merit of this system lay in the fact that the various phenomena of oxidation were for the first time grouped together and referred to the same agency.

The change of properties undergone by metals on calcination was a favourite subject of study with the phlogistic chemists. An equal share of their attention was devoted to the means of restoring to calxes (or oxides) the metallic properties. The phlogistic chemists recognized that the process of calcination of metals was essentially a process of burning. Now the char or substance left by the partial burning of animal and vegetable bodies constitutes the most generally applicable material for the preparation of the metals from their ores, or from the calxes. The calx or ore is mixed with charcoal, or with the analogons material coal, and these being heated together, the charcoal or coal is consumed, to all appearance ceases to exist, and a metal is produced from the calx.

It is to be observed that these changes only take place at a high temperature, and that the materials must therefore be heated. Now the primitive method of heating is to kindle a mass of vegetable matter such as wood, burning the material with a free supply of air. It was, however, found that for metallurgical operations it was more advantageous to employ the prepared material charcoal, instead of the crude material wood. In metallurgical operations the charcoal (or the analogous material coal) plays two parts, each of which is essential to the reduction of the ore or calx. The first of these functions is the production of a high temperature, the second that of acting on the heated ore so as to form a metallic substance. The charcoal employed as fuel has undergone combustion, and the charcoal mixed with the ore has apparently imparted to the ore the power of burning, that is to say, has formed from it a metal which is a material capable of burning. Coal, coke, wood charcoal, animal charcoal,

lampblack — in short, all the various forms of char — appeared to possess in an eminent degree what was termed the "principle of combustibility." The word *Carbone* was employed to denote the combustible *principle* contained in the various forms of char. The principle thus named is represented in terms of our modern views by what is known as a chemical element, and to this element the name *Carbon* is given.

The phlogistic chemists made no real advance towards explaining the reducing action of the various bodies containing the principle or substance Carbon. This explanation was reserved for Lavoisier, the great opponent of the phlogistic system. Previous to the work of Lavoisier, Black had prepared and studied the gas called "fixed air" (carbonic acid), which is obtained by the action of an acid on limestone. Lavoisier found that the same gas is formed when charcoal is burnt, and he further proved that in this process a constituent of the air (oxygen) unites with the charcoal, the fixed air or carbonic acid formed being composed of *Carbon* and oxygen. Lavoisier likewise proved that in calcination the substance he termed oxygen is abstracted from the air and combines with the metal, forming a calx or oxide. Finally, he showed that if a calx be heated with charcoal in the absence of air carbonic acid is formed, showing that the change which has taken place consists in removing the substance oxygen from its combination with the metal.

From this series of experiments it was evident that the *reducing* function of any form of char is better expressed by saying that the substance Carbon removes the substance oxygen than by saying that the combustible principle Carbon imparts to a calx the principle of combustibility. These two modes of expressing the same facts may be represented thus—

Calx *minus* the substance Oxygen = Metal, and
Calx *plus* the principle of combustibility = Metal.

From the date of Lavoisier's discovery the formation of carbonic acid has been invariably employed for detecting the presence of the element Carbon. Carbonic acid gas is readily recognized even in the smallest traces by the well-known property of throwing down a sediment of chalk when passed into lime water. Any substance which on burning forms carbonic acid contains, we say, the element Carbon. The amount of carbonic acid formed, which can be accurately determined, supplies the best means of estimating the weight of Carbon contained in any material. If a material when completely burnt yields carbonic acid *and nothing else* then we say that the material is composed wholly of the element Carbon. The purest wood charcoal when completely burnt yields no gas except carbonic acid, and no residue except a very small quantity of ash in which can be recognized those mineral ingredients which the plant from which the charcoal was prepared originally derived from the soil. Wood charcoal consists therefore almost wholly of Carbon. Any other form of char when freed as far as possible from foreign matter (*e.g.*, by drying to remove moisture) possesses the same, or nearly the same, physical constants as wood charcoal. The specific gravity is about 1·9. The materials are amorphous, or devoid of crystalline form.

It was not long after his discovery of the composition of carbonic acid that Lavoisier, in conjunction with certain other chemists, experimented on the combustion of the diamond. The diamond was placed in a glass vessel containing air, and the rays of the sun were concentrated to a focus on the diamond by means of a powerful lens. The diamond was by this means heated sufficiently to burn in the air of the vessel, and the gas evolved was collected over mercury, and tested. It was found to be carbonic acid. Soon afterwards (1796) an English chemist showed that the amount of carbonic acid formed by burning equal weights of diamond and of charcoal is the same, a conclusion which has since been repeatedly verified with the superior accuracy of modern methods.

Both diamond and charcoal are therefore said to consist wholly of the element Carbon.

In the year 1800, a third substance was added to the list of substances known as forms of Carbon. The mineral plumbago, or graphite, was formerly regarded as identical with molybdenum (a metallic sulphide), the appearance of each being similar, and both possessing the property of marking paper with a black streak, whence the name graphite (γράφω to write). It was shown, however, by Mackenzie, that graphite burns with formation of carbonic acid, the amount formed from a given weight of the material being the same in the case of charcoal and of diamond. Here, then, we have a third *form of Carbon*.

These three substances differ, in the first place, in certain important physical characters. The specific gravities are different, diamond standing highest in the list, and charcoal lowest. Diamond crystallizes in the regular or cubic system, graphite in the hexagonal system, whilst charcoal has no crystalline form or structure.

But it is not only in physical properties that the three substances differ; they differ to a certain extent also in chemical character. The temperature at which diamond burns is much higher than that at which the combustion of charcoal takes place. Thus, although oxygen unites with either substance, and in each case forms the same product, yet the readiness with which this combination takes place is very different in the two cases. In other words, the chemical relations of diamond and of charcoal, with respect to oxygen, are by no means identical. Again, graphite differs from either of the foregoing, in that the combined action of nitric acid and potassium chlorate convert it into a peculiar acid, a solid substance, known as graphitic acid.

In spite of these marked differences, chemical as well as physical, it is the universal practice to denominate all three substances, diamond, graphite, and charcoal, as "forms of the element Carbon," or *allotropic* modifications of Carbon. It must be confessed that the present phraseology is not as clear as might be desired, and is constantly a stumbling-block in the way of the tyro in chemistry. Set a schoolboy to write an essay on "allotropy" (or the existence of elementary substances in different "forms"), and he will choose as his example the element Carbon. He will begin by pointing out how widely different are the substances diamond, graphite, and charcoal, and will wind up his essay by saying that, notwithstanding these differences, "they are really the same thing—Carbon."

The source of confusion must be explained by reference to the atomic theory. We possess a great mass of evidence to show that what we observe in any chemical process is in fact the sum total as observed on the large scale of a number of phenomena, all precisely alike, occurring between the ultimate particles or chemical atoms of the substances. All the thousands of known substances are formed by various combinations of atoms of a comparatively small number of chemical elements. All the atoms of any one chemical element are exactly alike, but are different (*e.g.* in their mass) from the atom of any other element. A substance containing more than one kind of chemical atom is termed a compound substance or chemical compound. A substance containing only one kind of chemical atom is termed an elementary substance. Such a substance is diamond; it is formed wholly from atoms of one kind, from Carbon atoms. Charcoal likewise is

formed wholly of Carbon atoms. All Carbon atoms are, we believe, alike; but this by no means necessitates the identity of substances composed wholly of those atoms. We must look upon diamond and charcoal as *structures* both formed of the same material (the Carbon atom), but built up in different ways.

Regarded in this way the subject of *allotropy* is easy to understand, as easy as it is to understand, for instance, that a certain Jacobean house in Staffordshire is not a Norman castle though built of the sandstone blocks which once formed the feudal fortress originally standing on the same site. Sandstone is the matter of which both the house and the castle have been built, and in the same way diamond, charcoal, and graphite are constructed wholly of Carbon atoms; but to say simply and without qualification that diamond is Carbon, and that each of the other two substances is Carbon, is to employ language somewhat loosely and in a way which undoubtedly leads to confusion.

WHAT IS AN ANT?

By E. A. Butler.

Of late years the Ant, as everyone knows, has become the pet of the scientific world, and, to some extent, of the unscientific also. The fierce light of publicity has been brought to bear upon these little creatures, and upon their secret and subterranean doings. The laws which govern their communities, their common labours, their wars and foraging expeditions, their individual intelligence as manifested in their power of distinguishing their friends and detecting aliens, their vision and perception of colour, their senses of smell and hearing, their devotion to their young, their development and the duration of their life—these, and other such items, have been made the subject of observation and experiment, and the results have been eagerly read and discussed even in the daily press. But notwithstanding that Ants have become so famous, and their doings have been so minutely chronicled, there does not seem to be in the minds of the public generally a very distinct idea as to the identity of the creatures themselves, or, in other words, as to what insects are Ants and what are not. When an entomologist shows his collections to non-entomological friends, if he happens to have amongst his specimens any little dark-coloured, long-legged, wingless creatures, he is pretty sure to hear the suggestion hazarded in an inquiring tone that these must be Ants. Such a supposition will, it is likely, be quite as often wrong as right. The popular conception of an Ant is no doubt derived from the little black or dark-brown wingless individuals which one meets with everywhere, in our gardens and around our houses, quite as much as in the fields, lanes, or woods. But a conception which is formed merely by a random glance at such minute objects in rapid motion, and seen without the help of a magnifying glass, cannot but be vague in the extreme, and it is not surprising, therefore, that mistakes should frequently be made. The scientific idea of an Ant must be a good deal broader as well as a good deal more definite than this popular conception, and it is our purpose in this paper to show what are the distinctive characteristics of Ants, and how they can be distinguished from the numerous other insects to which they bear a superficial resemblance.

One cannot pronounce off-hand of any little dark-coloured, wingless, running insect, that it is an Ant, and, on the other hand, many true Ants would be neither dark-coloured nor wingless. Certain definite structural characteristics, which are accompanied with certain well-marked peculiarities of economy and habits, serve to distinguish Ants from other insects. Though what we have to say in this paper is intended only to apply to British Ants, it will be as well at the outset to correct a possible misapprehension, and to observe that there is a well-known tribe of insects which inhabit tropical regions, and no members of which are natives of Britain at all, that have unfortunately been called Ants though they are of an entirely different nature, whereby has resulted great confusion of popular zoological ideas. The insects in question are the so-called "White Ants," better named Termites, whose ravages are one of the greatest trials and annoyances of tropical countries. These destructive insects we have, quite apart from geographical limitations, nothing to do with here; zoologically they are not Ants at all: their structure is very different from that of the true Ants, and in many important respects their economy and habits are also strikingly dissimilar. The reader will therefore be good enough to exclude these creatures from his thoughts, and bear in mind that nothing that is said has any reference to them. The insects whose characteristics we have to consider are those which in this country are known as Ants or Emmets. We have between twenty and thirty kinds of them in this country, and these differ greatly in colour, ranging from the palest yellow, through various shades of red and brown, to deep jet black. Nevertheless there is a family likeness about them that renders them easily recognizable when once the distinctive points are known.

The first of these is to be found in the form of the insect. There is a large head (see Fig. 1), which is more or less abruptly cut off square behind, where it is often at its broadest. The head contains within it the brain and the muscles that move the jaws, in addition to the commencement of the digestive tract; and when we remember, in conjunction with this, the high degree of intelligence Ants manifest, and the muscular strength that is required for the hard work the jaws have to do, in fighting, in excavating, and in carrying heavy weights and unwieldy masses, often larger than the insect itself, we shall see very good reasons for the great size of the head, and shall naturally expect to find it, as is really the case, largest in those members of the community which have to do most of the work, whether mental or physical, viz. in the workers.

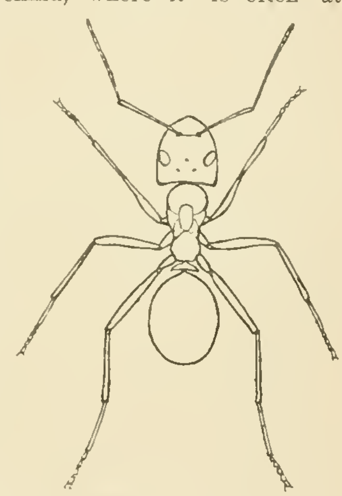


Fig. 1.—Worker of Wood Ant (*Formica rufa*).

The head is succeeded by a hump-backed thorax, often the narrowest part of the body, and showing very distinctly its composition out of three distinct segments. Then follows the abdomen, which is often extremely small in proportion to the other parts. Now it is in the construction of this part of the body that one of the most characteristic Ant-features is to be found. The front part of the abdomen is drawn out into a kind of thin stalk, which forms the connecting link between it and the thorax. But as this is the case with the majority of the Hymenoptera, to which order the Ant belongs, and gives them the narrow-waisted appearance which is familiar in wasps and ichneumon flies, it is not in the mere presence of this petiole, as it is called, that we find the distinguishing feature, but rather in its peculiar

form. The petiole is raised on its upper surface into one or two prominences, which have been called "nodes" or "knots"; the form of these, and the remarkable outline they give to this part of the insect, can best be seen by a side view (Fig. 2). The presence of these "knots" is one of the features by which an Ant may be recognized at once, and more than that, their number will determine to which of our two chief families any given specimen belongs. Omitting two rare insects which represent the family *Poneridæ*, but which are not likely to come in the way of the ordinary observer, and which, for practical purposes, may therefore be disregarded here, our British Ants are arranged in two families, the *Formicidæ* and the *Myrmicidæ*; the former, which contains the black and brown species we find in our gardens and streets, having only one "knot,"

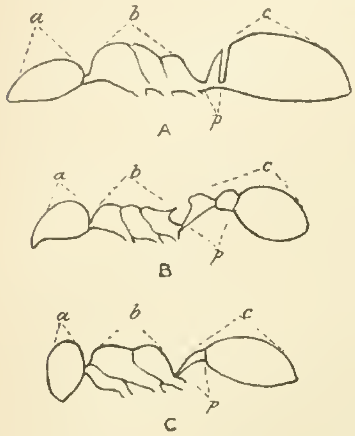


Fig. 2.—Side view of bodies of (A) *Formica rufa*, (B) *Myrmica ruginodis*, (C) *Pezomachus zonatus*. *a*, head, *b*, thorax, *c*, abdomen, *p*, petiole, with one "knot" in A, two in B, and none in C.

and the latter, which contains the red ones, having two. The petiole is movably jointed to the hinder part of the thorax, and hence the abdomen as a whole can be bent about this joint as upon a hinge. The extra joint in the petiole itself, in the *Myrmicidæ*, gives still greater mobility to the tiny oval abdomen, at the end of which is situated the sting, and apparently gives these creatures greater freedom in the use of that weapon than if they had only one knot. The *Formicidæ* do not sting, and are satisfied with an abdomen which is not capable of quite such extended movement. Though they do not actually sting, however, they are provided with an abundant supply of poison (formic acid), which they can eject at pleasure, and which is thus instilled into wounds made by the mandibles. If, for example, a nest of the great "Wood Ant," whose huge piles of sticks and fragments are familiar objects in woods, be disturbed, a strong smell resembling that of vinegar is perceived, and if the hand be brought near the opening, the insects rear up on their hind pairs of legs, open wide their jaws, tuck their abdomen between the hind legs so that its extremity points forward, and from this eject the poison with great force. The operator's hand soon experiences a smarting sensation resulting from the battery of formic acid brought to bear upon it, and if it then be touched with the tip of the tongue, a sharp sour taste will be observed. Sir John Lubbock states that he has experienced the effect of the formic acid upon the hand when held at a distance of as much as eighteen inches. If the head be held over a disturbed nest, a little distance above it, the atmosphere is found to be so impregnated with the fumes of the acid as to be almost overpowering.

Amongst the ichneumon flies and allied insects, there are to be found many species which are wingless, and which, as they run about on the ground, amongst dead leaves or other rubbish, or over the foliage of living plants, look a good deal like Ants. They may, however, be distinguished by an appeal to the knotted petiole test. The petiole is, indeed to be found in such insects, as their narrow waist at once testifies, but it has not the characteristic knots, as a glance at the accompanying figure (Fig. 2) will show. The insect whose body is represented in profile, and a portrait of which is given in Fig. 3, is a parasite on spiders, and has no connection with Ants, leading as different a life as could possibly be imagined.

The second feature of importance in distinguishing Ants from other insects is to be found in the antennæ. These are always of the same type. There is a little roundish joint by which they are attached to the head in the front of the face (Fig. 1). This is succeeded by a long slightly-curved shaft, occupying about a third of the length of the whole antenna; this is called the "scape." This, again, is succeeded by a string of small joints, the number of which depends upon the species and the sex; sometimes these increase in width a little before the tip, and so give a club-shaped appearance to this flexible part, which, from its flexibility as contrasted with the stiffness and rigidity of the scape, has been called the "flagellum" (*Latin*—whip), the lash, of which the scape is the handle. Now, bees and wasps also have antennæ constructed like this, but as they are never wingless, they are not likely to be mistaken for Ants, notwithstanding their "elbowed" antennæ. A greater difficulty will be felt in distinguishing winged Ants from wild bees, but here the scale or knot on the petiole will come to the student's assistance and settle the matter. If we look now at those other wingless Hymenoptera which are not Ants, we see that this antennal feature may again be used as a means of discrimination. It is true that in such insects the second oint of the antennæ is a good deal longer than any of the rest, and sometimes (especially in the very small species) even as long as in the Ants. In such cases the knotted petiole test must be applied. But in many instances the second joint is not nearly so long as in the Ants, and then the many-jointed terminal part is not placed at an angle to the rest, so that the antennæ do not become "elbowed" (Fig. 3). This is more evident in the living insect than in the dead one. The Ant's antennæ are carried pointing forwards, but with the flagellum set at an angle to the scape, like a human arm bent at the elbow, and then the whole organ and its two chief parts can be placed in the same variety of positions as the arm which it imitates; the antennæ of the *Pezomachus* above-mentioned, and other parasites are not carried bent in this way, but straight forward, and their tips are maintained in an incessantly quivering or vibrating condition, as the insect goes on its way. It is astonishing what differences of expression can be imparted to the head by the varying positions of the antennæ. The importance of this is seen when we bear in mind that an Ant's head, like that of any other insect, is covered with a hard, unyielding skin, any movement in which is absolutely impossible: all expression of the emotions, therefore, must be restricted to the movement of external parts, like the jaws and antennæ, and to the varying positions of the head itself; in fact, nothing more devoid of expression can be imagined than an Ant's head, apart from the jaws and antennæ; the fixity of the eyes and the bloated appearance of the head itself make it look as unintellectual as the helmet of a diver. And yet this expressionless object can have a strong semblance of an air of war-like courage and bold defiance, of intelligent appreciation and affectionate sympathy, of industrious

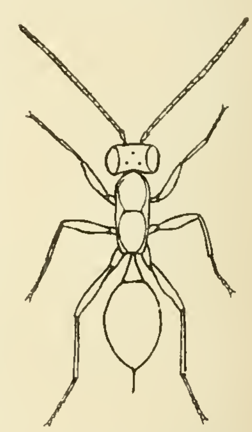
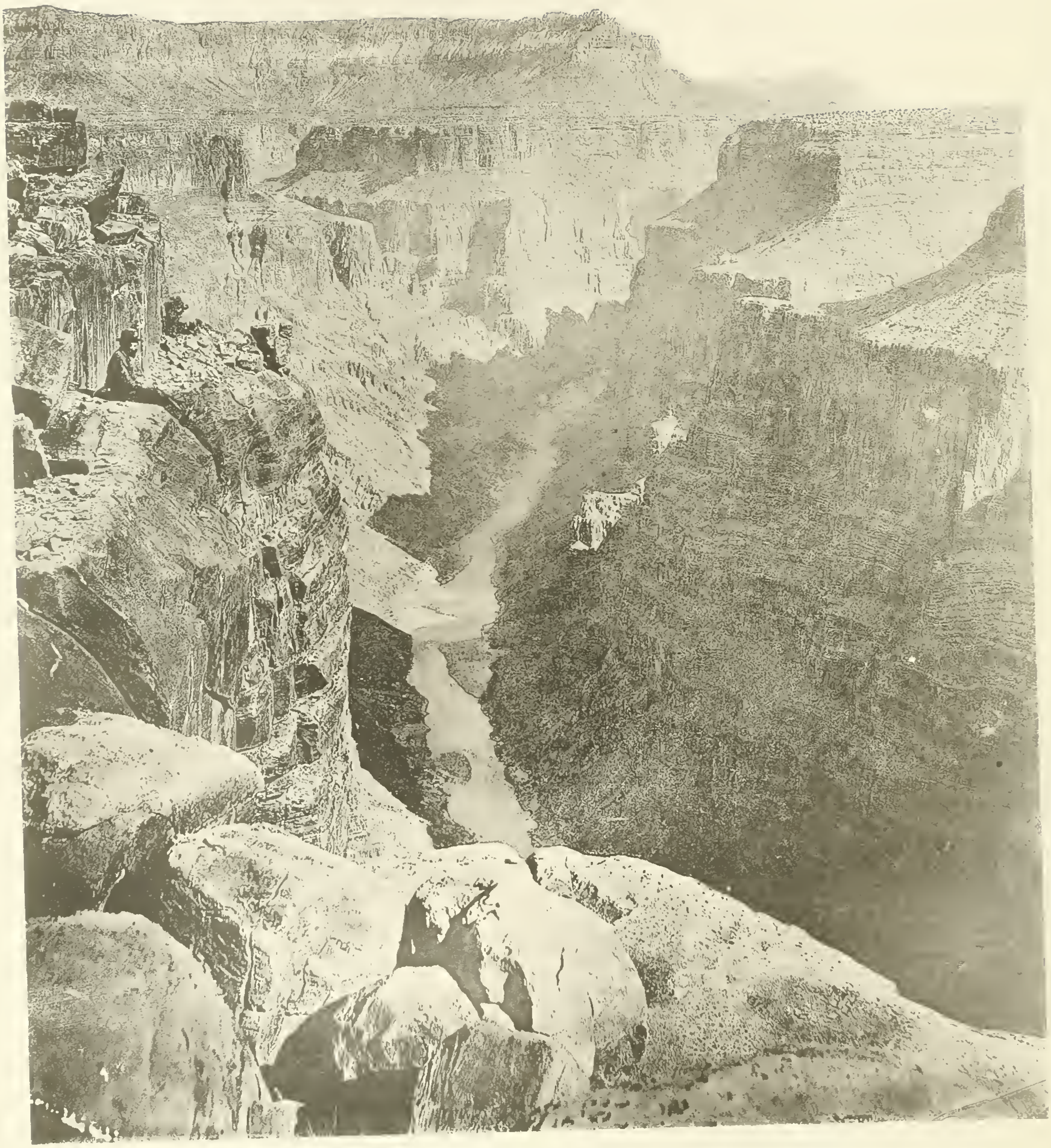


Fig. 3. — *Pezomachus zonatus*, a parasite on spiders; not an Ant.



Inner Gorge of Toroweap, looking East from near the summit of the Cliffs.

The Automatic Engraving Company, Strode Road, Willesden Green.

effort and fussy energy, imparted to it, simply by varying its position and by altering the attitude and motion of the jaws and antennæ.

Hitherto, we have been speaking only of the wingless forms of Ants, but these, though by far the most numerous, by no means constitute the whole of any given species. Every Ant exists in three forms, the male, the female, and the worker. Both the male and the female always have wings when first they assume the perfect form; the latter sex, however, retain them only till the marriage flight is over: they then voluntarily tear them off, so that in this sex the wings are only temporary appendages. These winged forms are seen for so short a time during any single season, that many people no doubt have never noticed them at all, and find it difficult to believe that such things exist. But even when one does see them, which will probably take place some fine day in August or September, it is often difficult to recognise them as having any connection with the wingless workers with which one is so familiar. It is clear that if two pairs of membranous wings, one large, the other small, be supposed to be added to a worker Ant, such an addition would of itself greatly alter the appearance of the insect. But this is by no means the only difference; there is also often a striking dissimilarity both with regard to size and colour, and the males, which are the smaller of the two, are frequently also much unlike their partners in colour and shape. For example, the little yellow Ant (*Lasius flavus*), which is abundant in many meadows and on heaths, making little hillocks, or taking advantage of the protection of a large stone or loose piece of rock, is yellow only in the worker; the male and female are both brownish black; or again, the little thin red Ant (*Myrmica rubra*), which is common everywhere, has a dark blackish brown male, which, in consequence of its wings and its deep colour, would be supposed to be a totally different insect from the worker, or the female. It is, then, only the workers that have no wings, or the females after they have mated, but then in this latter case the stumps of the wings may still be seen, whereas in the workers two little rounded points alone represent that portion of the larval structure which in the males and females developes into wings.

The development of wings in the male and female Ants has an important influence on the shape of the thorax, which adds another means of distinguishing the latter from workers when they have lost their wings. In the workers, the three segments of the thorax are not very unequal in size (Fig. 2), as each contains similar sets of muscles, viz., those for one pair of legs only; but when the wings are developed additional muscles are needed for these, and must be accommodated in the two hinder segments; hence the prothorax (first segment) becomes overlapped by the greatly enlarged mesothorax (second segment), which gives a still more humpbacked form to the insect, and the metathorax also (third segment) becomes enlarged. The enlargement is greatest in the mesothorax, as that segment has to carry the first pair of wings, which is by far the larger of the two, and is the chief instrument of flight. The wings are composed of transparent membrane, more or less clouded with a smoky tint, and strengthened by a few nervures, which enclose only a small number of cells.

Now the other wingless Hymenoptera with which Ants are often confounded do not exist in these three forms; all examples met with will be either males or females, no such things as workers being known, for a very good reason, viz.:—that there is no work for them to do. The insects do not form communities, but are, as already mentioned, piratical in habits, each managing its own affairs, which, in the case of the female, consist of little more than finding a suitable host in whose body she may deposit her eggs. These insects do not, as the Ants, couple in the air, and therefore never have any wings at all. It will be impossible, then, ever to find such insects in communities, or winged, in both of which respects they are to be distinguished from Ants. It should be observed, however, that some species, of which the females are always wingless, have winged males.

We are now in a position to answer our query "What is an Ant?" We have seen that Ants are Hymenopterous insects, which live in communities comprising three types of individuals, males, females, and workers; that the two former ones are winged and are to be met with only at a certain season in the year, but that the latter are never winged; that the antennæ are elbowed, and that the abdomen is attached to the thorax by a knotted petiole. And as they possess a poison gland and either a rudimentary or fully developed sting, they are referred to that section of the order called the "Aculeate" (sting bearing) Hymenoptera, which also contains bees, wasps, and some other insects. Lastly, they constitute a compact group of this section, to which the name Heterogyna has been given, in consequence of the great size and very different appearance of the females (*Greek*: dissimilar females).

THE CAÑONS OF COLORADO.

[*Second Paper.*]

By the Rev. H. N. Hutchinson, B.A., F.G.S.

(*Continued from page* 11).

CERTAIN important consequences follow from the rules about rivers, laid down at the end of our last paper. Thus, when rivers flow through or across mountain chains and plateaus, they must be older than these structural features. The elevation of a platform across the track of a river rarely diverts it from its course, because as fast as the ground under the river rises it cuts its way down through the obstruction. The region we are now considering offers a very complete illustration of the rules previously stated. We know that during the whole of Mesozoic time the watershed of the Colorado was submerged. In early Cainozoic (Lower Eocene) time it was a great freshwater lake. In due time this lake was drained, or emptied, presumably by the cutting down of its outlet as the country rose. By this process the present drainage system was begun, and every river must then have run in conformity with the features of the surface just exposed after rising above the waters of the lake, in which the Lower Eocene rocks were formed. But to-day we find that surface greatly deformed by displacements, and by erosion. The former made the "faults" and monoclinal flexures; the latter carved terraces out of the Eocene strata. The present courses of the rivers are not what they would have been if these features had then been in existence. They are entirely independent of them. They run in most cases against the slopes, and against the inclinations of the strata. They even cut through mountains and plateaus, enter cliffs and emerge from them; they flow over the monoclinal folds; they cross "faults" from the upthrow to the downthrow sides. As before stated, these facts can only be explained on the assumption that the rivers are older than the changes represented by all these structural features.

Many complex operations are involved in the evolution of the Grand Cañon, but the main factor is the erosion of its platform, and all the others are found to be bound up

with it. Mr. Powell, in his popular narrative of "Explorations of the Colorado River," has employed the term "base-level of erosion" to express an idea which is of great importance in physical geography. He was the first to give it that definiteness which it formerly lacked. The idea is this: when a smooth country lies only a little above sea-level, erosion takes place at a merely nominal rate. The reason of this is obvious, for the slopes being very slight, the velocities, and therefore the transporting powers of the streams are so feeble that they can do no more than urge along the detritus brought down from highlands round the margin of the country. Soil formed on slopes or mounds of the expanse is slowly carried off. The erosion is then said to be at its "base-level," or nearly so. If any given region of the earth remained for a long time at the same height above the sea, it would at last come to this state, and erosion would practically cease. Many regions have done so in the past, but the greater portion of the existing land of the globe has been subjected to repeated up and down movements. Were it not for such movements, the balance between land and water could not be maintained. If elevation took place at a rate faster than erosion could keep pace with, the seas would become dry land, and if subsidence went on everywhere faster than deposition takes place in the seas, the continents would disappear, and the globe would be covered with water. Suppose any region to have reached a base-level of erosion, if it be depressed the sea spreads over it, and it becomes an area of deposition. If, on the other hand, it is elevated, new energy is imparted to the streams, and erosion takes place more rapidly, because their slopes are increased, and so their powers of corrosion and of transportation become much greater. In this way new topographical features are carved out and long rapid slopes or cliffs are generated, and we have seen that such features are vigorously attacked on account of their height. During the progress of the Grand Cañon, its anterior spaces have for a time been at a "base-level of erosion." Throughout the Quaternary, and most of the Tertiary period, it has been rising; and the elevation varied from 11,000 to 18,000 feet, but the movements have not been uniform. It appears to have alternated between periods of activity and repose. One period of repose probably occurred late in the Miocene, or early in the Pliocene period. While it lasted the great Carboniferous platform (in which the chasms occur) must have been planed down to a very flat expanse, bounded of course by the Terraces. But since then a general upheaval of several thousand feet has taken place, giving a fresh impetus to the river.

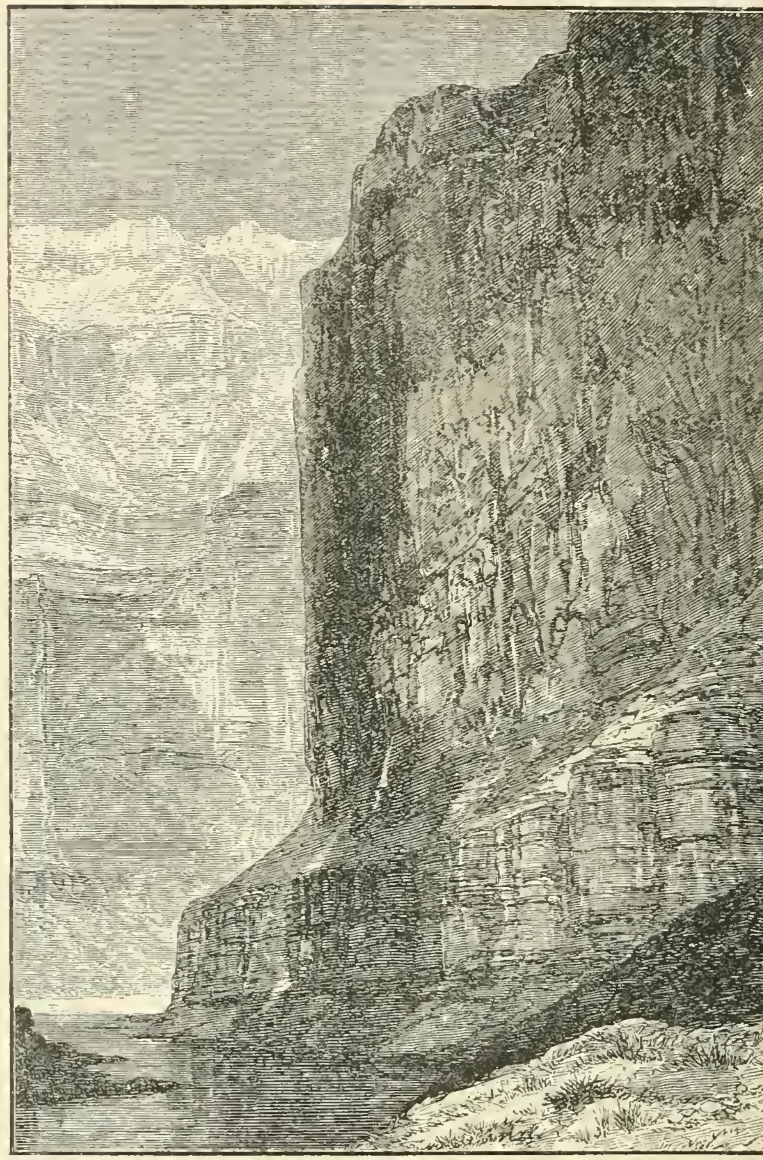


Fig. I.—The Marble Cañon.

Allusion may be made here to the volcanic phenomena of the Toroweap Valley, which runs along a "fault," whereby its western wall is made lower than the eastern one. Above and beyond is the Uinkarit Plateau, on the summit of which are a number of basaltic cones in perfect preservation. "Very many wide and deep floods of basaltic lava have poured over the edge of the plateau into the lower Toroweap Valley, and upon the great esplanade of the Cañon more than 1500 feet below, and spreading out into wide fields, have reached the brink of the inner gorge. Pouring over its brink, the fiery cascades have shot down into the abyss, and pursued their way many miles along the bed of the river. At one epoch they had built up the bed of the Colorado about 400 feet, but the river has scoured out its channel again and swept them all away, regaining its old levels; and is now cutting the sandstones below. The spectacle of the lava floods descending from the Uinkarit (plateau), as seen from 'Vulcan's Throne,' is most imposing."

There are about 120 distinct cinder-cones on the basaltic plateau of the Uinkarit, and probably many more volcanic vents. These often show a "linear arrangement" as if they were so many vents occurring along the course of a single fissure, but others, again, seem to be isolated in this respect. In Fig. II. volcanic dykes are seen (shaded dark). Then, the question arises whether they are connected with "faults," as might seem natural since "faults" are lines of weakness, up which steam and lava might find their way. Captain Dutton says, if there is any favoured direction taken by the volcanic forces, it would seem to be along the upthrow of the "fault," and a few miles from the line of dislocation. A truly wonderful displacement forms the western boundary of the Uinkarit

plateau. This is the great Hurricane "fault." Its full length is not known, but this enormous fracture extends at least 40 miles. In the southern wall of the Grand Cañon it appears with a displacement of about 1500 feet, throwing down the whole country to the west of it, and making a great cliff. The effect is as if a table were split across the middle and one side lifted up a foot or more. In one place the displacement is estimated at 12,000 feet: this is near the Valley of the Virgin, the Eocene strata on one side actually standing nearly 1000 feet lower than the much older (and therefore lower) Carboniferous rocks on the other side. Of the date at which this movement took place we shall speak later on.

The later geological history of the region of the Colorado plateau may now be briefly summed up. All the time when the great denudation was going on, by which the Mesozoic rocks overlying the platform of the Cañons were denuded and the Terraces formed, it may be concluded that the entire region occupied a level not much above the sea. It was at a "base-level of erosion." The rivers and streams no longer corroded their channels, but meandered slowly along the Carboniferous platform, dragging their burdens of soil along with them. This state of things was reached about the close of the Miocene period, and it lasted a considerable time. Then a new epoch of upheaval set in, and the work of cutting the Grand Cañon began. By this upheaval the country was hoisted up from 2000 to 3000 feet. Meanwhile a change of climate had taken place, the supplies of aqueous vapour failed, and the present dry time began, the rainfall being very slight. So most of the old streams dried up. Such streams as remained alive corroded their channels, but most of the platform suffered no more destruction than it does now by the slow waste that goes on. With this upheaval the Hurricane "fault" was developed, although it is possible that some slight displacement existed there already. If so, its magnitude was increased. At the same time the earliest volcanic outbursts took place. At length the uplifting paused for a time. Then the volcanoes ceased to work. The river for a time worked vigorously at the strata below it, the upward movement of the rocks having greatly increased both its corroding and transporting powers. But it quickly cut down until it found a new "base-level of erosion." Then corrosion ceased. During this second period of comparative repose the work of erosion was confined to the sapping of the newly-formed cliffs of the Cañon. The cliffs then began to recede away from the river; thus gradually was the broad avenue of the outer chasm made. When the cliffs of this outer chasm had receded two or three miles away from the river, another and more active period of upheaval set in. Once more the forces of elevation were brought into play, and this time the country was hoisted up more than before. Again the river began to corrode, or deepen, its channel. The displacements were increased in magnitude until the present "throw" of the "faults" was attained. These earth-movements affected the equilibrium between the internal and external forces at work on the so-called crust of the earth. The volcanoes became active again and poured out their lava streams. It was this second uplift that gave to the river the power to deepen its channel until it assumed its present condition. At present the elevating force, whatever it may be (and this is a question not yet settled), is inactive. Does it depend partly, as some think, on denudation? For it is conceivable that the removal of a considerable thickness of rocks from any given continental surface, may so diminish the downward pressure, due to their weight acting on the lower parts of the earth, as to disturb the equilibrium, and so force on an upward movement; but this is only a speculation. Anyhow, it seems that earth-movements and volcanic action are in some way connected together, for it is only during upheaval that volcanoes are active, and they are never associated with regions where depression is going on. The *active* volcanic regions of the globe at the present day are all regions of recent elevation, as may be seen by consulting a physical atlas.

This second upheaval was greater than the one which preceded it, amounting probably to 3000 or 4000 feet. The epoch at which it took place is no doubt a recent one, geologically. It probably began near the close of the Pliocene period.* At present

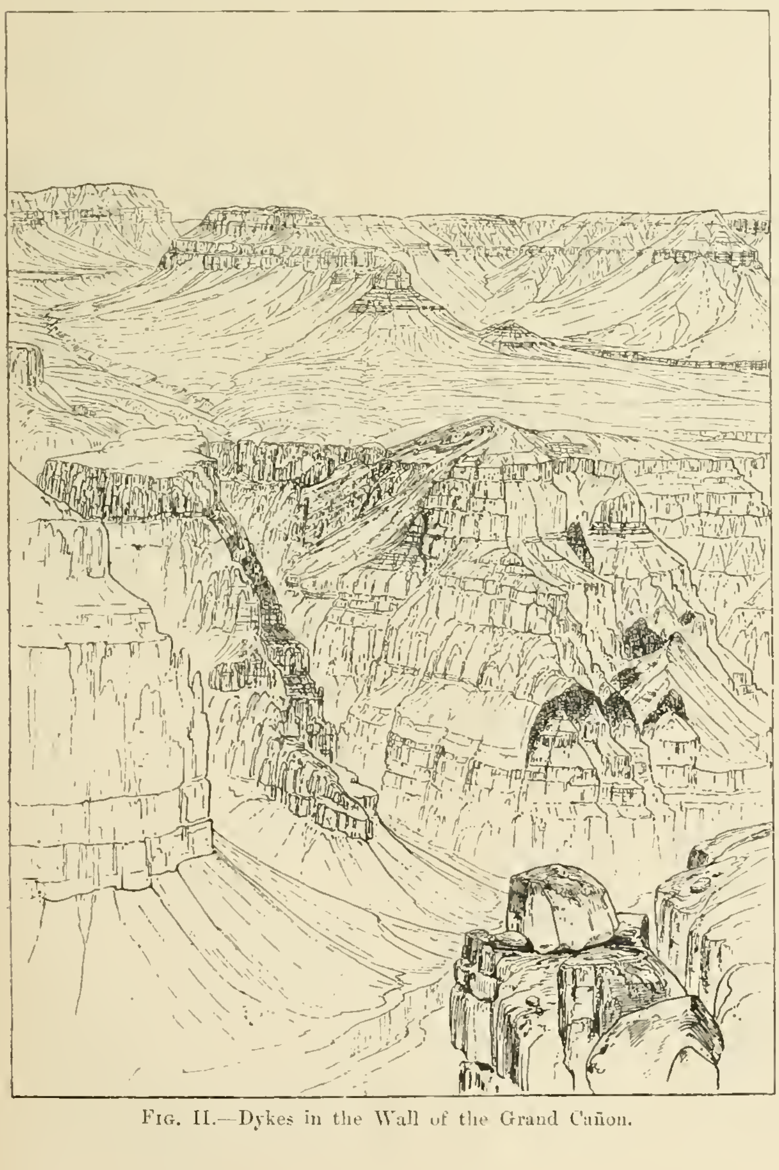


Fig. II.—Dykes in the Wall of the Grand Cañon.

* The Tertiary or Cainozoic era is divided by geologists into three periods. The oldest of these is the Eocene, then follow the Miocene and Pliocene.

no trace of movement can be detected, so it has probably ceased.

With regard to these displacements, there is some very interesting evidence which goes to show that these earth-movements took place slowly—in accordance with the "theory of uniformity" of the action of geological agents, which may be said to be the very basis of modern geology. The old theories of sudden catastrophies and revolutions have been superseded by the teachings of Hutton and Lyell.

One of these displacements, known as the Toroweap "fault," exemplifies this point very clearly; for excepting the dislocation itself, the "faulting" does not appear to have been accompanied by an injury to the strata. There is no trace of any shattering, crumbling, or smashing of the strata. All looks as clean and sharp as if it had been cut with a thin saw, and the smooth faces pressed neatly together. The plane of the "fault" is nearly vertical. A careful study of its surroundings shows that it is of recent occurrence. None of the "faults" of this region seem to have been produced in a violent manner, but to have been gradually developed through long stretches of time, inch by inch, or foot by foot. Many facts go to prove the modern character of this "fault," as also of the other one already alluded to, viz., the Hurricane "fault." In each case they cut through lava flows, which are evidently pretty recent; therefore they must be subsequent to the time when the flows took place. Mr. Powell has shown a connection between monoclinal flexures and "faults." They often shade into each other, and it looks as if the bending of the strata went on until a point was reached when the tension could no longer be borne, and a split was the result. This might aptly be illustrated by the long, sharp, and clean fractures that take place in a sheet of ice after a number of people have been skating on it. At first the ice only bends, but after a time the tension becomes too great and results in fractures.

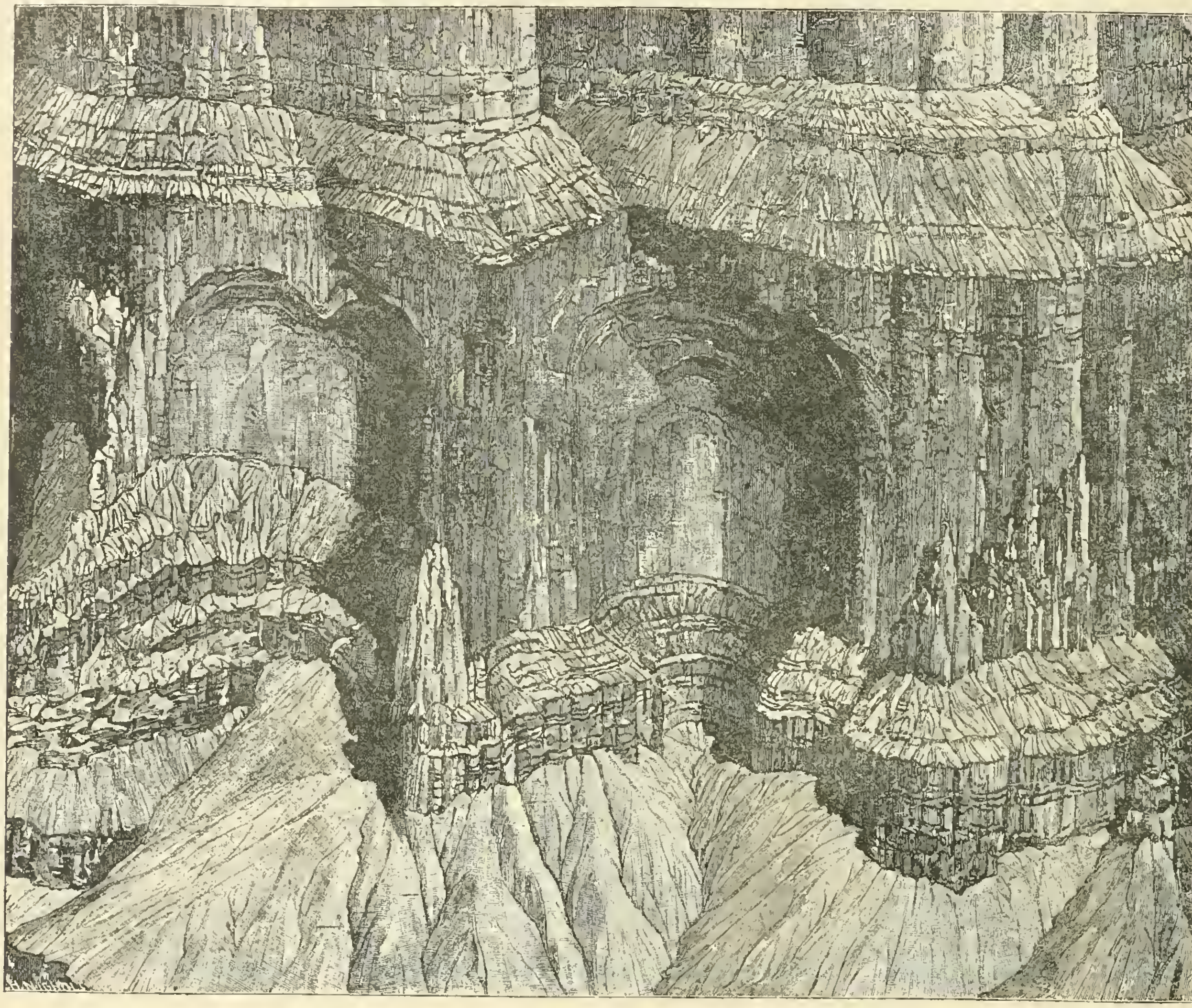


Fig. III.—Niches or Panels in the Red Wall Limestone, Grand Cañon District.

It is not necessary to describe in any detail the rocks of the Colorado plateau. Suffice it to say that the strata of each and every age are remarkably uniform over very large areas, especially in their lithological characters, and were deposited very nearly horizontally. In thickness and composition they are very persistent. The changes are always gradual. Local deposits, formed in small areas, are absent. They were all formed horizontally. The limestones are in great abundance in the Carboniferous rocks, but in the Mesozoic system limestones are rare. By far the greater part of the series of rocks exhibited in this

Map of the Cañon District. The shaded areas are covered with basalt. The residue of lava overflows from the N.E.

region is sandstone, in all its varieties. Sometimes it is quartzite, sometimes common sandstone in massive beds. Shales, however, abound in the Permian and Trias. These pass into the marly beds of the Cretaceous and Eocene. Thus we see that, while the strata are remarkably uniform in their horizontal range, they vary very much in vertical range, producing layers that differ very much in hardness, compactness, and solubility. Obviously the results of the attack of erosion on all these different rocks will be very different. These important facts have had a great influence in determining the architectural features of the cliffs and their profiles.

The upper tributaries of the Colorado River have their sources in lofty regions at a distance which are abundantly watered. But the main stream and its lower tributaries flow through regions which are exceedingly dry, so that nearly all the water flowing through the Grand Cañon comes from the highlands far away—the Uinta and the Rocky Mountains (*see* atlas). Though numbers of waterways open into the Cañons, very few of them carry living streams, the rest only convey spasmodic floods for a few days or hours when snows melt or showers and storms prevail. The rainfall is very slight. For the region draining laterally into the Glen and Marble Cañons it may be estimated at four inches per annum; but when rainstorms do come their consequences are very striking. The rills and washes are thick with mud and sand, and their waters loaded to their utmost capacity. There is nothing to hold the earthy matters together, so that the instant a rill is formed it is a rill of mud. Rills and streams gather together with marvellous rapidity, plunging furiously along. Although the rainfall on the middle and lower levels—about 5000 feet above sea-level—is on the whole quite small, the transporting power of such water as runs into the river is very great; a cubic yard of running water in the plateau country probably carries several times more sediment than the same quantity of water in rivers, such as the Mississippi, that run into the Atlantic. There are two causes for this remarkable difference: First, the comminuted *débris* of the plateaus due to "weathering" is not held together by vegetation, but lies loosely on the rocks and slopes of loose stones (called *taluses*); secondly, because the slopes are always very great, and we have already pointed out that the transporting power of streams is enormously increased by an increase in the declivity, as well as corroding power, it follows that much more solid matter is brought down. Soil and vegetation exist only in moist regions where a good deal of rain falls throughout the year, and they retard the work of erosion by forming a covering that protects the rocks below. They also tend to store up water and so equalize the flow of rivers and streams throughout the year, thus preventing the rapid floods we have alluded to above. The direct effect of increased rainfall is to increase erosion, but its indirect effect, through soil and vegetation, is to retard it. With regard to the slopes or gradients of the River Colorado, they vary considerably. The length of the part we are now considering is about 218 miles, and the average fall 7·56 feet per mile. But it varies from 3 or 4 to 21 feet per mile. The river is still sinking its chasm in the strata, though a great part of the river bed is over bare rock, and wherever this is so corrosion is proceeding rapidly. Its great cutting power is due to the large quantity of sand which it carries and the high velocity due to steep gradients. It only remains to notice a very curious phenomenon in the Red Wall Limestone, viz. the numerous niches or panels (see Fig. III.). Of these there are literally hundreds along the extent of the limestone front, and, as far as is known, they are seen in no other member of this series of strata. Captain Dutton says he is unable to explain the cause of this persistent phenomenon, and is very much perplexed by it. We can only suggest that they may have been dissolved out by the action of percolating water. It used to be thought that the Cañons might originally have been due to great cracks which the river has deepened and widened, but on reviewing carefully the mass of evidence brought forward in Captain Dutton's able monograph we find absolutely no trace of evidence for this idea. "Faults" and cracks could never have made such a great network of valleys.

Letters.

[The Editor does not hold himself responsible for the opinions or statements of correspondents.]

A LAW IN LOGARITHMS—AN ERROR OF VEGA'S.

To the Editor of Knowledge.

Dear Sir,—It is of course widely known that the logarithms of numbers cannot be *always* calculated from tables by means of proportional parts, and that the degree of accuracy extends merely to a certain number of places. But it is *not*, I think, nearly so well known that the accuracy increases with the *squares* of the numbers. Thus suppose we are given ·0004340775 for the logarithm of 1001, and we calculate the logarithm of 10005 therefrom, by means of proportional parts, the result will be ·0002170387 . . . ; the arithmetical mean between the logarithm of 1000 and the logarithm of 1001. But the real logarithm of 10005 is ·0002170929 . . . ; and the logarithm calculated by proportional parts is too small by ·000000054 . . . If in the same way we calculate the logarithm of 100005 from that of 10001, we shall find the result is too small by ·00000000054; and so on, the error decreasing by two zeros for every increase of one figure in the number. The same law holds good with all intermediate numbers, and 0000000054 (8 zeros) is the error in the logarithm of 31625 if calculated by taking the arithmetical mean of the logarithms of 3162 and 3163. 3·162 . . . being the square-root of 10, 3162 is of course a geometrical mean between 1000 and 10,000; and therefore the error rightly has 8 zeros before the 54.

Curiously this law of error was overlooked by Vega in his splendid reprint of Vlacq's "Logarithms" to 10 places. Wishing to facilitate the correct calculation of intermediate logarithms to 10 places, he gave a table of what he called "second differences," which were to be added to the results taken by means of proportional parts. But omitting to take notice that these "errors" or "second differences" decrease inversely as the *squares* of the numbers, his table, throughout the greater part of it, makes the corrections greater than they should properly have been. T. S. Barrett.

THE CAUSES OF THE ICE AGE.

To the Editor of Knowledge.

Sir,—I am sure that my friend Sir Robert Ball would not have made the charges which he has made against Sir John Herschel and Dr. Croll without good reason, though I am not aware of the passages on which it is based. But his article seems to imply that the general reasoning of both authors rests on this erroneous assumption, which is certainly not the case. Both assume (as indeed every writer on the subject must do) that each hemisphere receives more heat during its summer than during its winter. This is clearly implied, for instance, in 368*a* of Herschel's *Outlines*; but neither he nor Dr. Croll, I think, have anywhere attempted to compute the relative quantities of heat which we receive during our summer and our winter. This, however, was done in a very complete manner by Dr. Haughton in *The Transactions of the Royal Irish Academy* for 1881. His figures for the entire hemisphere agree pretty well with Sir Robert Ball's, but they vary with the latitude of the place.

Sir Robert Ball is probably correct in setting down the effects of the solar heat as equivalent to a mean elevation of temperature of more than 300° F. This elevation, if distributed in the ratio of 63 to 37, would give a mean elevation of 378° F. in summer and 222° F. in winter, with an annual range of 156° F. Of course, nothing like this annual range exists, the reason being that modifying causes (especially air-currents and ocean-currents) come into play. Now unless it can be shown that these modifying causes do not come into play in the case of the secular variation arising from changes in the eccentricity of the earth's orbit, the amount of the secular variation must be proportionally reduced. This, accordingly, is Dr. Haughton's conclusion. For lat. 50° he makes the annual variation of temperature 44·2° F., and the secular variation only 3·685° F., more than one-half of which will be represented by a summer elevation of temperature instead of a winter depression. Sir Robert Ball's reference to the large variation in the interior of continents is, I may remark, not much to the purpose, since it is not in the interior of continents but in islands and continental countries not far from the shore that the traces of glaciation are most evident; and in Siberia, for instance, where this great annual range of temperature exists, there is, generally speaking, no permanent snow-cap.

But behind all this lies the further question, Is such an unequal distribution of temperature favourable to the production of a *permanent* snow-cap? Dr. Croll's arguments on this point seem to me unsatisfactory. Sir Robert Ball (as far as I am aware) has not argued it at all. Suppose, as an extreme case, that the winter 37 per cent. of heat was distributed over 364 days in the year, and that we then received the remaining 63 per cent. on the 365th day. Would this 63 per cent. of the total heat be incapable of melting the snow-cap produced by the 364 cold days? I think not. In our latitudes 63 per cent. of the total annual heat would suffice to melt at least 50 feet of solid ice. It would no doubt have to raise the ice from a low temperature to freezing point before the melting commenced; but then, without a great increase in our rainfall or snowfall, it would not have 5 feet of ice to melt instead of 50. The snow might be more than 5 feet deep, but it would not be equivalent in amount to more than 5 feet of ice. In fact, a given quantity of heat would, it seems to me, produce its maximum melting effect when brought to bear in the shortest time. Suppose the heat received at a certain place is sufficient to maintain a constant temperature of 30° F. If the distribution is uniform, and the locality is sufficiently supplied with moisture, there will be a permanent snow-cap; but, unless the snowfall is very large, could I not melt the snow-cap once a year if allowed to distribute the heat as unequally as I chose?

This seems to me to be the most important question to be decided. As to Dr. Croll's theory of the diversion of the Gulf Stream, the fact that glaciation extends much farther to the south on the American than on the European side of the Atlantic affords pretty strong evidence that the Gulf Stream was not diverted during the Glacial Period.

Dr. Haughton takes occasion to point out that the secular range of temperature could not account for the miocene coal-beds of North Grinnell Land; but the possibility, or impossibility, of accounting for this and other evidences of a former mild climate in the Arctic regions is regarded by Dr. Croll as a crucial test of his theory. I remain, yours faithfully,

Dublin, Jan. 13th, 1892. W. H. S. Monck.

[One of the difficulties to my mind in accepting the explanation of the cause of the Glacial Epoch suggested by Dr. Croll, and adopted by Sir Robert Ball, is that, if we could rely on such calculations, Mars should be permanently covered with snow, whereas we see its white polar caps wax and wane with the Martian summer and winter, proving, it seems to me, that there must be very potent modifying influences (such as the existence of a dense atmosphere) which upset the assumption that the mean temperature of a planet's surface may be assumed to vary inversely as the square of the planet's distance from the sun. Our atmosphere is probably not constant in quantity through geologic periods. Its gases are, we know, continually being absorbed by one set of chemical actions, and set free by others. The one set of actions may exactly balance the other, but if not, the atmosphere must be growing or decreasing in amount, and this would profoundly modify the surface temperature. So also would the elevation of an ocean bed which turned aside a tropical current such as the Gulf Stream.—A. C. Ranyard.]

Dr. Max Wolf is continuing his photographic explorations of the Milky Way. In a letter received three or four weeks ago he informs us that several of his negatives show the tracks of meteors which have been observed to pass across the region photographed during the exposure. This gives the hope that in the future rich meteor showers may be observed photographically and the area of the heavens from which they radiate more exactly mapped than hitherto.

Prof. W. W. Payne, of Northfield, Minnesota, and Prof. George E. Hale, of Chicago, have published the first number of a new astronomical journal, which gives promise of occupying a very high place in astronomical literature. Prof. Hale states that he has undertaken the work at the instigation of his friend, Prof. C. A. Young, who

suggested to him the importance of establishing a periodical devoted to spectroscopy and astro-physics. He was loth to multiply the number of astronomical journals, and it was ultimately decided that the new periodical should be published in conjunction with the "Sidereal Messenger," which now changes its name to "Astronomy and Astro-Physics," and will appear ten times a year under the joint editorship of Profs. Payne and Hale. The subscription will be four dollars per annum.

Notices of Books.

Memory: Its Logical Relations and Cultivation. By F. W. Edridge-Green, M.D., F.G.S.; 2nd Edition. (Baillière, Tindall and Cox.) According to the author, memory is of two classes. There is sensory memory, dealing with the impressions received by the mind either from the external world or from its own processes, and motor memory, dealing with the motor impulses resulting therefrom—a classification based on the same principle as that of nerves into sensory and motor. Memory as a whole he treats as a distinct and definite faculty of the mind, having for its seat a limited portion of brain tissue; the sensory division he localizes in the *optic thalami*, and the motor in the *corpora striata*, the physiological basis of the memory being in each case a modification of the protoplasm of the cells of the nervous centre. Using the term "memory" in the broadest and most general sense of retentiveness, the faculty which is the foundation of all stores of knowledge and experience, he very fully and ably illustrates its differences in kind by reference to many of the most ordinary details of everyday life, showing that he has been a close and thoughtful observer of human nature. The idea which runs through the book is, perhaps, best expressed in brief by the following sentence: "The centre for sensory memory is so arranged that every impression received through a lifetime is registered in a definite position and order of sequence, from the first moment of a child's life to the day of his death, and all sensations, perceptions, and ideas received at the same time either form component parts of one impression or closely associated impressions." The memory, of course, consists in the revival of these impressions. The theoretical part of the subject is fully elaborated in a series of clearly written and closely reasoned chapters, which are extremely interesting reading whether one accepts the conclusions or not. But the book has more than a merely academic interest, great though that may be; Dr. Edridge-Green is intensely practical as well, realising as he does the extreme importance to its possessor, in the complex life of to-day, of a memory which can always be relied upon; and he applies his principles to the construction of a series of rules by the observation of which the memory may be cultivated and improved. These show a clear appreciation of the causes of the defects which are most commonly experienced, and though couched in somewhat technical language, they are sufficiently explained and illustrated by actual examples which are of familiar occurrence, and there will therefore be little difficulty in understanding their drift, and in putting them into practice. Like most other writers on memory, the author has a mnemonic system of his own, of special use as an aid in the memory of numbers; this, if somewhat cumbrous and troublesome, will no doubt yield good results to those who try it with enthusiasm, though we are of opinion that each person will generally profit most by a system either of his own devising, or at least of his own adaptation.

Handbook of the London Geological Field Class. (George Philip & Son.) This is a record of five years' work by the above class, which, under the presidency and personal conduct of Professor H. G. Seeley, has utilized Saturday afternoons during the summer months in studying geology at first hand. Excursions are made to localities which can be reached by a railway ride of not more than an hour from London; the professor explains on the spot the geographical and geological features of the locality visited, and the students, in their turn, under the professor's guidance, observe and chronicle matters of geological interest, collect fossils, &c., and are thus put in training which may, if they choose to follow it up, eventuate in their becoming practical geologists. By this excellent plan employment is found for the Saturday holiday, which is at once recreative, health-giving, and instructive; habits of scientific accuracy are encouraged by the constant appeal to Nature, which cannot but be of the highest educational value. The handbook is compiled by members of the class, under Professor Seeley's supervision, and in it we have abstracts of the professor's lectures and reports, between 80 and 90 in number, written by the students in description of the localities visited, and illustrated with sketches and sections from their own note-books. The whole forms a neat little pocket volume, which will be not merely of interest to the members of the class, but may be usefully employed as a guide-book by any amateur geologists who wish to study the cretaceous and tertiary deposits of the South East of England.

THE RELATIVE BRIGHTNESS OF THE PLANETS.

By J. E. Gore, F.R.A.S.

THAT the planets shine with very different degrees of brightness is a fact familiar, perhaps, to most people. The great brilliancy of Venus, when favourably situated as a morning or evening star, is well known, and has frequently given rise to the erroneous idea that a new celestial visitor had appeared in the sky. Jupiter, when in opposition to the Sun and high in the heavens, as it is in some years, also forms a brilliant object in our midnight sky, and it is closely rivalled in lustre by the "red planet" Mars, when nearest to the Earth, as it will be in the autumn of the present year. The difficulty of detecting the planet Mercury with the naked eye, owing to its proximity to the Sun, is well known. When seen, however, under favourable conditions, this planet shines with considerable brilliancy, but, as it can only be seen at its brightest for a few days in the morning or evening sky a little before sunrise or a little after sunset, and then only for a comparatively few minutes in the twilight, it generally escapes the observation of the casual observer. The "ringed planet" Saturn usually appears brighter than an average star of the first magnitude, and may be easily distinguished by its dull yellow colour. The light of this planet is of course considerably increased when the ring system is widely open, the bright rings being very luminous; but, when the rings are nearly invisible, as they are at present, the brightness of Saturn is much reduced. Uranus is just visible to the naked eye on a clear night when its exact position with reference to neighbouring stars is known, but Neptune is quite beyond the range of unaided vision.

These differences in the relative brightness of the planets are due to four causes: (1) The distance of a planet from the Sun; (2) the distance of the planet from the Earth; (3) the size of the planet; and (4) the reflecting power

of its surface, or the "albedo." Of these the first three are easily determined by observation, and a simple method of computing the relative albedos of the different planets forms the subject of the present paper.

The method of computation is as follows. The brightness of two planets will vary inversely as the square of their distance from the Sun, and *directly* as the size of the planets' discs as seen from the Earth, or, making due correction for their crescent and gibbous forms, as the square of their apparent diameters measured in seconds of arc. The results of this calculation will represent the relative brightness the two planets should have if both had the same albedo. If, however, one of them appears brighter than calculation indicates it implies that its reflecting power or albedo is greater than the albedo of the other. As the relative apparent brightness can be measured with a photometer, we have all the necessary data for calculation of the relative albedos.

The albedo is generally represented as a decimal fraction. This fraction denotes the proportion of light reflected compared with the amount received; the albedo of a surface reflecting *all* the light which falls upon it would be represented by unity. Probably, however, no such surface exists, the "albedo" of even freshly fallen snow being less than unity.

The difference of albedo in the planets is in some cases very striking. In 1878, when Mercury and Venus were in the same field of view of the telescope, Nasmyth found that Venus was at least twice as bright as Mercury. He compared Venus to clear silver and Mercury to lead or zinc. From photometric observations by Pickering and Zöllner, the brightness of Venus is nearly as great as if its surface was covered with snow, and Zöllner found that the surface of Mercury is comparable with that of the Moon, which has a small albedo. This difference of surface brightness is very remarkable when we consider that Mercury is much nearer to the Sun than Venus. If we suppose that the surface of Venus is covered with a cloudy canopy, as has been suggested, this cloudy covering would perhaps account for the planet's great reflecting power.

Owing to the uncertainty which exists as to the relative apparent brightness of Venus and Mercury as viewed with the naked eye, it is not easy to compute correctly their relative albedos. Olbers found Venus at its greatest brilliancy 19 to 23 times as bright as Aldebaran, but Plummer estimated it as nine times brighter than Sirius, which would make it 56 times brighter than Aldebaran. Mercury is perhaps about equal to Aldebaran when at its greatest brilliancy. I compared the planet and the star in June, 1874, in India, and found them about equal.

Assuming that when Venus is at her greatest brightness she is distant from the Sun 66 millions of miles, and that in this position she subtends an angle of 40 seconds of arc, and taking the corresponding quantities for Mercury as 28 millions and $8\frac{1}{2}$ seconds respectively, I find that Venus should appear about four times brighter than Mercury. Taking Venus as 20 times brighter than Aldebaran we have the albedo of Venus equal to five times that of Mercury. Zöllner found for Mercury an albedo of 0·13. My calculation would, therefore, make the albedo of Venus equal to $0{\cdot}13 \times 5$ or 0·65. Zöllner found 0·50. The data used in the above computation are too uncertain to yield an accurate result.

For the planets outside the Earth's orbit, let us take Mars as our standard. For this planet Zöllner found an albedo of 0·2672, or about double the albedo of Mercury. For the minor planets we have hardly sufficient data to enable us to compute their albedos; these little planets being so small that the apparent diameters of their discs cannot be accurately measured.

Comparing Mars and Jupiter, we have the mean distances from the Sun represented by the numbers 1·523 and 5·20. Their surfaces are therefore illuminated by sunlight in the inverse ratio of the squares of these numbers. That is, the solar illumination on Mars is to the solar illumination on Jupiter as the square of 5·20 to the square of 1·523, or as 27·04 to 2·32; and the apparent diameter of Mars at mean opposition may be taken at 18 seconds of arc, while that of Jupiter is 46 seconds. Hence the illuminated surface of Jupiter is $(\frac{46}{18})^2$, or 6·53 times that of Mars. The relative brightness of the two planets should therefore be $\frac{27{\cdot}04}{2{\cdot}32 \times 6{\cdot}53}$, or 1·78; that is, Mars should be 1·78 times brighter than Jupiter. Now Pickering found the stellar magnitude of Jupiter, when in opposition, to be 2·52, or about $2\frac{1}{2}$ magnitudes brighter than the zero of the scale of magnitudes, and that of Mars 2·25. This makes Jupiter 1·2823 times brighter than Mars. But we have seen that Mars should be 1·78 times brighter than Jupiter. Hence Jupiter is $1{\cdot}78 \times 1{\cdot}2823 = 2{\cdot}2825$ times brighter than it should be had it the same albedo as Mars. The albedo must therefore be $0{\cdot}2672 \times 2{\cdot}2825 = 0{\cdot}609$. Zöllner found an albedo of 0·72, but Bond computed that Jupiter emits more light than it receives from the Sun (Chambers' "Descriptive Astronomy," 3rd edition, p. 117). This would suggest that the planet shines with some inherent light of its own, a conclusion which has been also arrived at from other considerations.

In the case of Saturn the existence of the bright rings complicates the observations of the planet's brightness. Pickering's photometric measures make it about equal to a star of the first magnitude when in opposition and the rings invisible. Mars is therefore 3·25 magnitudes, or about 20 times brighter than Saturn. Now the relative distances of Mars and Saturn from the Sun are represented by the numbers 1·523 and 9·539. The squares of these are 2·32 and 90·99, which implies that the intensity of the solar light on Mars is 39·2 times that on Saturn. Taking the apparent diameter of Mars at 18 seconds and that of Saturn at 19 seconds, we have the apparent surface of Mars $(\frac{18}{19})^2$ or $\frac{324}{361}$ that of Saturn. Mars should therefore be $39{\cdot}2 \times \frac{324}{361}$, or 35·17 times brighter than Saturn. But it is only 27 times brighter. Hence the albedo of Saturn must be greater than that of Mars in the ratio of 35·17 to 27, or the albedo of Saturn $= \frac{35{\cdot}17}{20} \times 0{\cdot}2672 = (0{\cdot}47)$. Zöllner found 0·4981. I am inclined, however, to think, from my own observations, that Saturn, when in opposition and shorn of his rings, is slightly brighter than a star of the first magnitude. If this be so the albedo would have a somewhat higher value than that just computed.

Coming now to the planet Uranus we find the highest albedo of all the planets. Zöllner found 0·64, or slightly greater than that of Jupiter, but I find a still higher value. The relative distances of Mars and Uranus from the Sun are 1·523 and 19·183. The squares of these numbers are 2·32 and 367·99. Hence the intensity of the solar illumination on Mars is $\frac{367{\cdot}99}{2{\cdot}32}$, or 158·6 times that on Uranus. Taking the apparent diameter of Uranus at 4 seconds, and that of Mars at 18 seconds, as before, we have the area of the disc of Mars $(\frac{18}{4})^2$, or 20·25 times that of Uranus. Hence Mars should exceed Uranus in brightness $158{\cdot}6 \times 20{\cdot}25$ or 3211·65 times, if both planets had the same albedo. Now Zöllner found the stellar magnitude of Uranus to be 5·46; Pickering finds 5·56, and my own eye observations make it about 5·4. We may therefore safely assume its brightness at 5·5 magnitude. This gives a difference of 7·75 stellar magnitude between Mars and

Uranus, and implies that Mars is 1259 times brighter than Uranus. But we have seen that Mars should be 3211·65 times brighter if the surfaces of the two planets had the same reflecting power; hence it follows that the albedo of Uranus must be $\frac{3211 \cdot 65}{1259}$, or 2·55 times greater than that of Mars. We have, therefore, the albedo of Uranus $=0{\cdot}2672 \times 2{\cdot}55 = 0{\cdot}68$, or nearly equal to that of white paper, which is 0·70.

Let us now consider the planet Neptune, for which Zöllner found an albedo of 0·46. The relative distances of Mars and Neptune are 1·523 and 30·054. This gives the solar illumination on Mars 389·32 times that on Neptune. Taking their apparent diameters at 18 seconds and 2·9 seconds respectively, we have the result that Mars should be 14,996·6 times brighter than Neptune. Now Pickering found the stellar magnitude of Neptune to be 7·96, which makes Mars 10·21 magnitudes, or 12,023 times brighter than Neptune. Hence we have the albedo of Neptune $=\frac{14996 \cdot 6}{12023} \times 0{\cdot}2672 = 0{\cdot}333$, a result in striking contrast to the albedo found above for Uranus. I think there can be no doubt that Uranus has the highest albedo of all the planets of the solar system. Comparing it with Jupiter I find, by the same method of computation, that the albedo of Uranus = albedo of Jupiter × 1·213. Hence with Zöllner's value of Jupiter's albedo, 0·62, we have the albedo of Uranus 0·75, a very high value indeed, exceeding that of white paper, which is 0·70, and pointing strongly to the conclusion that Uranus is in a highly heated condition, a conclusion which seems to be partly supported by the evidence of the spectroscope.

To further test the high albedo of Uranus, let us compare the relative brightness of Uranus and Neptune. According to Pro. Pickering's photometric measures, Uranus is 5·56 magnitude and Neptune 7·96. Uranus is therefore 2·4 magnitudes. or 9·12 times brighter than Neptune. The relative distances of the two planets from the Sun being 19·183 and 30·054, we have the intensity of the solar light on Uranus 2·4545 times that on Neptune. But the areas of the discs are as 4^2 to $(2{\cdot}9)^2$, or as 16 to 8·41. Hence, the brightness of Uranus should be $\frac{16}{8 \cdot 41} \times 2{\cdot}4545$, or 4·67 times that of Neptune. Hence it follows that the albedo of Uranus must be $\frac{9 \cdot 12}{4 \cdot 67}$, or 1·9528 that of Neptune. Assuming Zöllner's value of 0·46 for the albedo of Neptune, we have the albedo of Uranus $= 0{\cdot}46 \times 1{\cdot}9528 = 0{\cdot}898$ (!) Even with the low value of Neptune's albedo, which I have found, viz., 0·333, the albedo of Uranus would be $0{\cdot}333 \times 1{\cdot}9528 = 0{\cdot}65$, a value which still makes its albedo the highest of all the planets.

It is difficult to say what the albedo of the Earth itself may be. Possibly it does not differ much from that of the planet Mars. The Moon's albedo is rather low, 0·1736, according to Zöllner. It is, however, greater than that of Mercury, which seems to have the smallest reflecting power of all the planets.

With reference to the satellites, those of Mars are so small that we have no data for computing their albedos. Prof. Pickering's estimates of their diameter were made on the assumption that their albedos do not differ much from that of Mars itself.

Assuming a diameter of 3400 miles for the third satellite of Jupiter, the largest and brightest of the system, and the mean diameter of Jupiter itself at 87,000 miles, we have the area of Jupiter's disc 655 times that of the satellite. If both have the same albedo, Jupiter should therefore be 655 times brighter than the satellite. Now Pickering finds the stellar magnitude of this satellite to be 5·21. This makes Jupiter 7·76 magnitudes or 1271 times brighter than the satellite. Hence the albedo of Jupiter must be nearly twice that of the third satellite.

The diameter of Saturn's largest satellite, Titan, is somewhat doubtful, but assuming it at 3000 miles, and its stellar magnitude to be 9·43, as measured by Pickering, the diameter of Saturn being 72,000 miles, I find that the albedo of Saturn would be 2·2 times of Titan. This would make the albedo of Titan about 0·21, but owing to the uncertainty which exists as to its diameter this result must be considered very doubtful.

The satellites of Uranus and Neptune are so faint that no satisfactory results could be computed. For the satellite of Neptune Pickering finds a stellar magnitude of 13·82, or 5·93 magnitudes fainter than its primary. If we take the diameter of Neptune at 36,000 miles, and assume that its albedo is twice that of its satellite, I find that the diameter of the satellite would be about 3300 miles. Assuming the same albedo, the diameter would be about 2340 miles.

PERIODICAL COMETS DUE IN 1892.

By W. T. Lynn, B.A., F.R.A.S.

Dr. S. Oppenheim has recently published in the *Astronomische Nachrichten* (No. 3064) the result of an investigation of the orbit of the fourth Comet of 1886, which was discovered by Mr. W. R. Brooks at Phelps, N.Y., on the 22nd of May in that year. He finds that the most probable length of its period is 5·6 years, and as it passed its perihelion in 1886 on the 6th of June, another will be due in the present month of January. Dr. Oppenheim thinks, however, that the Comet will not become visible unless the perihelion passage occurs considerably later in the year than this.

Failing this, the only known Periodical Comet due to return in 1892 is that of Pons-Winnecke, which was also last seen in 1886, when it was detected by Mr. Finlay at the Cape of Good Hope on the 9th of August, and passed its perihelion on the 16th of September. The first certain discovery of this Comet was made by Pons at Marseilles on the 12th of June, 1819, but it appears probable that it was observed by Pons himself early in February, 1808, though the observations made on that occasion were too few and doubtful (partly on account of the close neighbourhood of several nebulæ) to furnish the means of determining its orbit with any accuracy. It was after the return of Pons's Comet of 1819 in 1858, when it was re-discovered by Prof. Winnecke on the 8th of March, and passed its perihelion on the 2nd of May, that it was recognized as taking its place amongst the Periodical Comets, with period of about 5·6 years. It was not, however, seen at the next return, which must have taken place about the end of 1863, but was observed at the returns of the summer of 1869 and the early spring of 1875. At the return in 1880 it was unfavourably placed and again escaped observation, but (as already mentioned) it was observed again in the latter part of the summer of 1886, and will be due once more early in the present year.

In the year 1880 the late Prof. Oppolzer, of Vienna, made some calculations which appeared to indicate that this Comet was undergoing an acceleration of its period, and he suggested that this might be due to the effect of a resisting medium in space acting upon its motion, as Encke had thought he had obtained decisive evidence in the case of the Comet now always called by his name. But since the time of Encke the diminution of the periodic time in the latter Comet has proved to be not constant, so that the probability of the resisting medium explanation no longer holds. And in the case of the Pons-Winnecke

Comet, since its return in 1886, Dr. von Haerdtl, of Vienna, has made a re-investigation of its motions and found no evidence of any such effect of a supposed resistance. The suggestion, however, will give some additional interest to observations of the Comet at its approaching return to perihelion.

THE FACE OF THE SKY FOR FEBRUARY.

By Herbert Sadler, F.R.A.S.

The Sun's disc, when visible, should be examined for spots and faculæ. The following are conveniently observable minima of some Algol-type variables (*cf.* "Face of the Sky," for January). U Cephei.—February 4th, 5h. 55m. p.m.; February 9th, 5h. 34m. p.m. Algol.—February 2nd, 5h. 41m. p.m.; February 19th, 10h. 36m. p.m.; February 22nd, 7h. 23m. p.m. λ Tauri.—February 1st, 10h. 11m. p.m.; February 5th, 9h. 3m. p.m.; February 9th, 7h. 55m. p.m.; February 13th, 6h. 48m. p.m.; February 17th, 5h. 40m. p.m.

Mercury is a morning star during the first portion of the month, but owing to his great southern declination and proximity to the Sun is very badly situated for observation. He rises on the 1st at 6h. 46m. a.m., or 56m. before the Sun, with a southern declination of 22° 26′, and an apparent diameter of $5\frac{1}{2}''$, $\frac{82}{100}$ths of the disc being illuminated. On the 6th he rises at 6h. 52m. a.m., or 42m. before the Sun, with a southern declination of 21° 44′, and an apparent diameter of 6″, $\frac{85}{100}$ths of the disc being illuminated. After this date he is too near the Sun to be visible. Venus is an evening star, and is now becoming a conspicuous object in the western sky. On the 1st she sets at 7h. 47m. p.m., 3h. 1m. after the Sun, with a southern declination of 7° 14′, and an apparent diameter of $12\frac{1}{2}''$, $\frac{84}{100}$ths of the disc being illuminated. On the 29th she sets at 9h. 15m. p.m., with a northern declination of 7° 18′, and an apparent diameter of $14\frac{1}{4}''$, $\frac{76}{100}$ths of the disc being illuminated, and the brightness of the planet being rather less than one-half of what it will be at its greatest at the beginning of June and middle of August next. Venus is in conjunction with Jupiter at 10h. 14m. a.m. on the 6th, the geocentric distance separating the limbs of the two planets being only $18\frac{1}{2}''$, but the phenomenon occurs after sunrise in Europe. The two planets, however, will present a most beautiful appearance in the western sky in England on the evenings of the 5th and 6th. At 7h. p.m. on the 5th Venus will be about 38′ *s.p.* Jupiter, and at the same hour on the next evening she will be about $21\frac{1}{2}'$ *n.f.* Jupiter, the field of view on both evenings, with a low power, being a singularly pretty one. During the month Venus passes from Aquarius into Cetus, but without approaching any conspicuous star. Mars is, for the purposes of the observer, invisible; and as Uranus does not rise till after midnight at the beginning of February, we defer an ephemeris of this planet until next month.

Jupiter is still visible, close to the S.W. horizon, but he is so rapidly approaching the Sun that our ephemeris of him only extends over the first third of the month. On the 1st he sets at 8h. 16m. p.m., $3\frac{1}{2}$h. after the Sun, with an apparent equatorial diameter of $34\frac{1}{3}''$, and a southern declination of 5° 8′. On the 10th he sets at 7h. 52m., or 2h. 49m. after sunset, with an apparent equatorial diameter of $33\frac{3}{4}''$, and a southern declination of 4° 20′. The following phenomena of the satellites occur while Jupiter is more than 8° above, and the Sun 8° below, the horizon. On the 1st a transit egress of the shadow of the fourth satellite at 6h. 7m. p.m. On the 2nd a transit egress of the shadow of the first satellite at 5h. 52m. p.m. On the 4th a reappearance from occultation of the third satellite at 6h. 51m. p.m., and its eclipse disappearance at the same instant. On the 8th a transit egress of the second satellite at 5h. 54m. p.m.; while visible he describes a short direct path in Aquarius.

Saturn is an evening star, rising on the 1st at 9h. p.m., with a northern declination of 2° 19′, and an apparent equatorial diameter of 18·6″ (the major axis of the ring system being 42·8″ in diameter, and the minor 2·5″). On the 29th he rises at 7h. p.m., with a northern declination of 3° 3′, and an apparent equatorial diameter of 19·1″ (the major axis of the ring system being 44″ in diameter, and the minor 1·9″). The following phenomena of the satellites may be observed (the times are given to the nearest quarter of an hour):—February 2nd, $4\frac{1}{2}$h. a.m., Tethys, eclipse disappearance. February 3rd, 10h. p.m., Dione, eclipse disappearance. February 4th, 2h. a.m., Tethys, eclipse disappearance. February 5th, $11\frac{1}{4}$h. p.m., Tethys, eclipse disappearance. February 8th, $1\frac{1}{2}$h. a.m., shadow of Rhea in central transit. February 9th, $0\frac{1}{2}$h. a.m., shadow of Titan in central transit; 4h. a.m., Titan in inferior conjunction with centre of Saturn, 9·9″ south. February 10th, $5\frac{3}{4}$h. a.m., Rhea, eclipse disappearance. February 12th, 3h. a.m., Dione, eclipse disappearance. February 17th, $2\frac{1}{2}$h. a.m., shadow of Rhea in central transit; $4\frac{1}{4}$h. a.m., Titan, eclipse disappearance. February 19th, $4\frac{1}{2}$h. a.m., Tethys, eclipse disappearance. February 20th, 2h. a.m., Iapetus in inferior conjunction with the centre of the planet; Tethys, eclipse disappearance. February 22nd, 11h. p.m., Tethys, eclipse disappearance. February 23rd, 2h. a.m., Dione, eclipse disappearance. February 24th, $8\frac{1}{2}$h. p.m., Tethys, eclipse disappearance; $11\frac{1}{2}$h. p.m., shadow of Titan in central transit on Saturn. February 25th, $1\frac{3}{4}$h. a.m., Titan skirts southern limb of planet. February 26th, $3\frac{1}{4}$h. a.m., shadow of Rhea in central transit. During the month Saturn describes a short retrograde path in Virgo, without approaching any naked-eye star.

Neptune is still visible during the working hours of the night, rising on the 1st at 11h. 40m. a.m., with an apparent diameter of 2·6″, and a northern declination of 19° 48′. On the 29th he rises at 9h. 39m. a.m., with a northern declination of 19° 49′. During February he is almost stationary just north-west of ε Tauri, and about the middle of the month he will be observed to be about 80″ *n.f.* a $9\frac{1}{4}$ magnitude star.

There are no well-marked showers of shooting stars in February.

The Moon enters her first quarter at 9h. 39m. a.m. on the 5th; is full at 7h. 38m. p.m. on the 12th; enters her last quarter at 0h. 15m. a.m. on the 21st; and is new at 3h. 47m. a.m. on the 28th. She is in perigee at 9·2h. a.m. on the 1st (distance from the earth 226,765 miles); in apogee at 9·8h. a.m. on the 17th (distance from the earth 251,845 miles); and in perigee at 11·8h. a.m. on the 28th (distance from the earth 223,660 miles). The greatest western libration is at 2h. 46m. a.m. on the 9th, and the greatest eastern at 9h. 48m. p.m. on the 23rd.

Chess Column.

By C. D. Locock, B.A.Oxon.

All communications for this column should be addressed to the "Chess Editor, *Knowledge Office*," and posted before the 10th of each month.

The solution of the Four-Move Problem in the January number is necessarily withheld till its publication in the *Chess-Monthly*.

Correct Solutions have been received from W. T. Hurley, Giu. Pianissimo, M. B. (Jesmond), and A. Rutherford, who are to be congratulated on their success in mastering the intricacies of this most difficult problem. Duals, which were most abundant, of course did not count; otherwise even Giu. Pianissimo, whose analysis was most exhaustive and logical, would have missed one or two points. None of the four survivors have suggested an immediate division. An attempt therefore will be made to combine their various suggestions as far as possible, on the chance of a separation. For this purpose the following difficult problem is given; and, as an additional test, another problem has been posted (January 24th) to each competitor. In this latter problem the number of moves is not given, and duals, etc., in leading variations will each score one point, the same being deducted for each incorrect claim. Solutions of both problems should be sent in by February 9th, and the analysis in each case should be continued up to White's third moves.

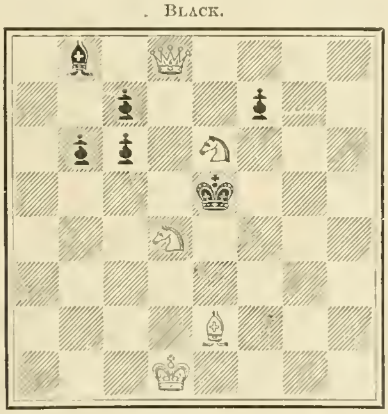
A. Rutherford.—Thanks for the correction. The mistake is explained below.

R. W. Houghton.—The problem was a very difficult one; to be beaten by it is no disgrace, though certainly unfortunate.

SECOND TIE PROBLEM.

[Also from the *Chess-Monthly* Tournament.]

Black.



White.

White to play, and mate in four moves.

CHESS INTELLIGENCE.

The Steinitz-Tschigorin Chess Match in Havana began with the year. The match goes to the player who first wins ten games, draws not counting. Mr. Tschigorin won the first game; the second and third were drawn; Mr. Steinitz won the fourth, and, after another draw (56 moves), the sixth in 48 moves. The first four games averaged only 31 moves each. So far the Russian player has consistently adopted his favourite Evans Gambit, which Mr. Steinitz has defended by 7. . . . B to KKt5 and 8. . . . Kt to KB3, no longer apparently relying on his expensive defence Q to B3. In the other games he has played the common or German form of the Ruy Lopez, with the exception of the sixth game, which was a Two Knights' Defence.

The *Pittsburg Dispatch* announces a Three-Move Problem Tournament (direct mates only). Competing positions, with motto, solution, and sealed name and address, must be mailed (in Europe) not later than March 1st, and addressed to "*Chess Editor, Pittsburg Dispatch*, P.O. Box 463, Pittsburg, Pa.

The National Masters' Tournament of the British Chess Association will probably be held at the British Chess Club some time this month. The programme will be issued shortly.

The following is one of two Consultation Games, played on December 12th last by telephone, between the Liverpool Chess Club and the British Chess Club, London. The former were represented by Messrs. Burn, Dod, Rutherford, and Wellington; the London players being Messrs. Guest, Hirsch, Locock, and Mundell.

[Vienna Opening.]

White. (B.C.C.)	Black. (Liverpool)
1. P to K4	1. P to K4
2. Kt to QB3	2. Kt to KB3
3. P to B4	3. P to Q4
4. P×KP	4. Kt×P
5. Kt to B3 (*a*)	5. B to K2 (*b*)
6. P to Q4	6. P to QB4 (*c*)
7. B to Q3	7. Kt to QB3
8. Kt×Kt	8. P×Kt
9. B×P	9. Kt×QP
10. B to K3 (*d*)	10. B to B4 !
11. B×B (*e*)	11. Kt×B
12. Q to K2	12. Castles
13. Castles	13. Q to Kt3
14. P to B3	14. QR to Qsq (*f*)
15. B to B4	15. R to Q2
16. Q to K4	16. Q to K3 (*g*)
17. QR to Ksq	17. R to Ksq
18. Kt to Kt5 (*h*)	18. B×Kt (*i*)
19. B×B	19. Kt to K2
20. B×Kt	20. QR×B
21. Q to QR4 (*j*)	21. P to QKt3
22. R to K2	22. Q to Q4 (*k*)
23. R to Qsq	23. Q to Kt2
24. R to Q6	24. P to KR3
25. Q to B2 (*l*)	25. Q to Bsq
26. Q to Q3	26. Q to KKt5
27. R to Q8 (*m*)	27. Q to K3
28. R×R	28. R×R
29. Q to R6 (*n*)	29. R to K2
30. P to QKt3	30. R to Q2 (*o*)
31. P to KR3	31. P to KKt4
32. Q to R4	32. P to KB4
33. K to R2	33. K to B2 (*p*)
34. P to KKt4	34. P×P
35. P×P	35. K to Kt2 (*q*)
36. Q to K4	36. Q to Q4 (*r*)
37. Q×Q	37. R×Q
38. P to B4 !	38. R to Q2
39. K to Kt3	39. K to Bsq
40. K to B3	40. K to K2
41. R to B2	41. K to K3
42. R×P ch	42. K×P
43. R to Kt6 (*s*)	43. R to Q7
44. R×Pch	44. K to B3
45. R to B5ch	45. K to Kt3
46. K to K4	46. R×P
47. K to Q5	47. R to QKt7 (*t*)
48. R to B3	48. K to Kt4
49. K to B6	49. K×P
50. R to Q3 (*u*)	50. K to B5
51. K to Kt7	51. K to K5
52. R to R3	52. R to QR7
53. R to Kt3	53. K to Q5

And after a few more moves the game, which had lasted over seven hours, was given up as drawn.

Notes.

(*a*) 5Q to B3 is more usually played; Black has three good defences in QKt to B3, or Kt×Kt, or P to KB4. The London representatives selected the text move on the ground that it had not been much analyzed.

(*b*) If 5 . . . B to QKt5 White intended to reply 6. B to Q3!, or if 5 . . . B to KKt5, 6. Q to K2!, which is much better than 6. B to K2.

(*c*) An excellent reply, and we believe new at this stage. If instead 6 . . . Castles, White gets the superior game by 7. B to Q3.

(*d*) This move was the subject of much discussion, some of the players preferring 10. Castles. The simple move Kt×Kt would have given Black an isolated Pawn in return for their own, but was rejected as too unenterprising.

(*e*) If 11. Q to Q3, Kt×Ktch; 12. P×Kt, Q×Q; 13. B×Q, B×B; 14. P×B, Castles (QR); 15. K to K2, R to Q4; 16. P to KB4, KR to Qsq, with some advantage in position.

(*f*) After 14 . . . Kt×B, 15. Q×Kt, Q×P; White recovers the Pawn by 16. KR to QKtsq. Black prepares, instead, for doubling his Rooks on the King's file.

(*g*) The best way of defending the Knight, which now threatens to escape at Q3. Q to Kt3 was not so good; White might win a Pawn at once by Q to R4.

(*h*) With a view to simplicity, as there is not much chance of attack. B to Kt5 instead would be met by B to Bsq, threatening P to B3.

(*i*) Best. If 18 . . . Q to Kt3; 19. P to K6, B×Kt; 20. P×Pch, Q×P; 22. Q×Rch, Q×Q; 23. R×Qch, K to B2; 24. B×B and wins. Black's next move is also much better than Kt to Q3, which would ultimately leave the Knight out of play.

(*j*) The first of an interesting series of moves, by which White keep their King's Pawn indirectly guarded even against the constantly threatened . . . P to B3.

(*k*) If P to B3, 23. P×P. Black manœuvre to get their R at K2 and the Q behind it.

(*l*) Still preventing R×P. Q to KKt4 or Q to QB4 also have merits. Black's next move prevents Q to B5.

(*m*) A hasty move, made under pressure of the time limit. They should have played P to KR3 first, when the Queen would have nothing better than a return to QBsq.

(*n*) Black threatened P to B3, but perhaps K to B2 was a better defence.

(*o*) Preventing the escape of the Queen at Q3. If instead 30. . . . P to B3; 31. P×P, Q×R; 32. P×R, etc. After their next move White offered a draw, which was declined.

(*p*) K to Kt2 was probably better, *vide* their 35th move. On their next move 34. . . . P to B5 would of course be answered by 35. Q to K4.

(*q*) Before making this move Black in their turn proposed a draw, but withdrew the offer while White were consulting.

(*r*) If 36. . . . R to KB2; 37. R to Q2, R to B5; 38. Q to Kt7ch and draws; but the game should be drawn anyhow. After the exchange of Queens Black cannot attack the King's Pawn with both King and Rook.

(*s*) Playing now to win, but they should have been content to draw by K to K3. If then 43. . . . R to Q5; 44. R to R7.

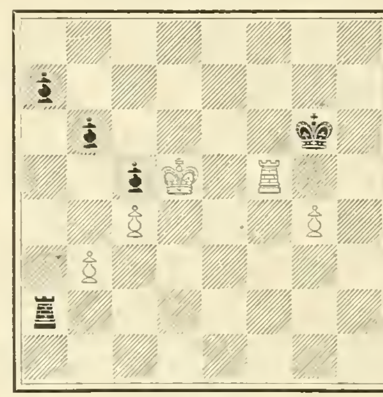
(*t*) They might play 47. . . . R to R6 (see diagram). If then 48. R to B3, P to R4; 49. K to B6? (not so good as K to K4 which seems to draw); 49. . . . P to R5; 50. P×P! (if 50. K×P, R×Pch wins); 50. . . . R×R; 51. K×P, R to B3ch?; 52. K to Kt5! (the only move. If 52. K to Kt7, R to B5 wins; or if 52. K×P, K to B2 wins). White now apparently draws at least by going straight on with the Rook's Pawn; but Black at move 51 should have played R to QR6! which apparently wins, as pointed out by Mr. Hoffer; but it is very difficult.

(*u*) R to QB3 would have saved a move, but the game is easily drawn.

Position after White's 47th move.

(LIVERPOOL.)

Black.



White.

(LONDON.)

In the other game, which will be given next month, the Liverpool representatives were Rev. J. Owen, and Messrs. Howard, Kaizer, and Cairns. The name of the latter gentleman bears a certain telephonic resemblance to that of Mr. Burn, hence our mistake last month in stating that Mr. Burn took part in both games. We are indebted to Mr. A. Rutherford, of Liverpool, for this information.

Contents of No. 75.

TERMS OF SUBSCRIPTION.

"Knowledge" as a Monthly Magazine cannot be registered as a Newspaper for transmission abroad. The Terms of Subscription per annum are therefore as follows:—To any address in the United Kingdom, the Continent, Canada, United States, Egypt, India, and other places in the Postal Union, the Subscription is 6 shillings, including postage; or 1 dollar 50 cents; or 6 marks; or 7 francs 50 cents.

For all places outside the Postal Union, 5 shillings in addition to the postage.

Communications for the Editor and Books for Review should be addressed, Editor, "Knowledge" Office, 326, High Holborn, W.C.

KNOWLEDGE
AN ILLUSTRATED
MAGAZINE OF SCIENCE
SIMPLY WORDED—EXACTLY DESCRIBED

LONDON: MARCH 1, 1892.

CONTENTS.

BRITISH MOSSES.

By the Rt. Hon. LORD JUSTICE FRY, F.R.S., F.S.A., F.L.S.

(*Continued from page* 25.)

IF the reader will now return to my table A, at the beginning of this article, he will see that I have given some account of all the Musci except the Anomaleæ; these are a somewhat heterogeneous group of plants, of great interest to the botanist, but with which I fear to detain my reader lest I should disgust him with apparently dry details.

Sphagnaceæ.—Next in order to the Musci in my table A will be found the Sphagnaceæ, or Turf or Peat Mosses, a natural group of comparatively few species and very marked organization. The general appearance of this class of Mosses may be gathered from the figure of one already given (see Fig. 16), and is well known to almost everyone who has had any interest in a hot-house.

Vast tracts of land in this country and throughout Northern Europe and America are covered with plants of this group, and large tracts which are now fertile agricultural land, where they have entirely ceased to grow, have in former times been occupied by them. The bogs of Ireland, which are mainly constituted of Turf Moss, were computed in 1819 by the Bog Commissioners to occupy 2,830,000 acres. No Moss has probably ever, at least in the present state of the globe, played so large a part as the Sphagnum or Turf Moss.

Structure.—It is to the peculiar structure of the Peat Moss that this great part on the theatre of the globe is to be attributed.

Leaves.—In the young leaves the component cells are all alike; then by a differential growth we are presented with large cells (sometimes of a square or rectangular shape) surrounded by narrower cells; then chlorophyll forms in these narrow cells, but is absent from the square cells; from these the contents disappear, and water or water-like fluid occupies the whole cell; subsequently annular and spiral threads develop on the walls of the square cells. The intimate structure of the leaf thus enables it to absorb great quantities of water.

But again, the shape of the leaves is in many species adapted to the retention of water. By a retardation of the lateral as compared with the mesial growth, the leaf assumes a boat shape. Often the edges of the leaves are turned over; the leaf thus affords means of holding water.

Figs. 27 and 28 will enable the reader to follow

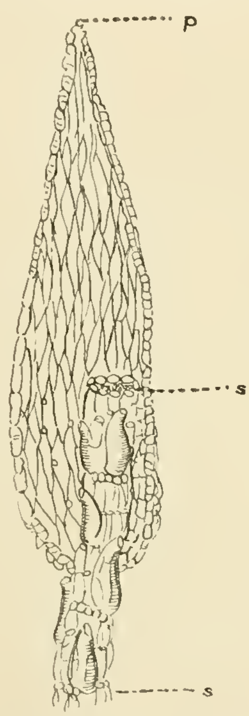


FIG. 27.—Leaf of *Sphagnum acutifolium*, magnified; *s s*, stem; *p*, point of the leaf. After Schimper.

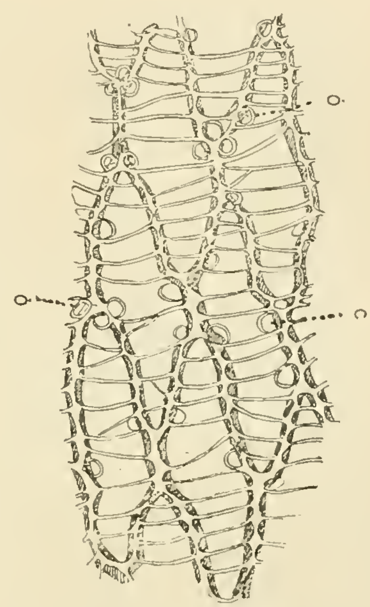


FIG. 28.—Portion of leaf of *Sphagnum acutifolium*, highly magnified; *o o*, orifices opening from interior of cells. After Schimper.

the foregoing description. Fig. 27 shows a magnified leaf of the *Sphagnum acutifolium*—with a portion of the adjoining stem (*s s*), of which more hereafter. The edges of the leaf are turned over—as may be seen by looking at the extreme point of the leaf (*p*) where these foldings over cease. Fig. 28 exhibits a portion of a leaf far more highly magnified; the large cells free from chlorophyll bounded by the narrower cells charged with it will be at once observed as well as the spiral threads developed on the walls of the larger cells, a peculiarity of the leaf of this genus which enables one to detect the presence of its remains so long as any organic structure is retained; and at the points *o o* are seen orifices opening from the interior of the cell and admitting water.

A reference to Fig. 16 will show that the leaves of the Sphagnum are borne on lateral branches. These at the

Fig. 29.—Lateral branches of *Sphagnum*; *s s*, stem; *h h*, horizontal branches; *p p*, pendant branches. After Schimper.

head of the plant form a thick and often widely extended tuft; but lower down the branches grow out laterally from the stem, generally in tufts of four branches, of which, as shown in Fig. 29, two generally grow out more or less horizontally, and two are disposed in close proximity to the stem, round which in fact they fall (see again Fig. 16), so as to exert a great capillary attraction and keep a great mass of water in suspension even against the force of gravity.

Yet further, as it would seem, to add to the absorptive capacity of the leaves, Nature, in one or more species of the Peat Moss, has recourse to a further expedient. Round the base and sides of the leaf, clusters of half free cells, with spirally marked walls, are clustered, ready, like their sister cells in the leaf, to carry their full complement of water. Such a group is shown (magnified of course) in Fig. 30.

The stem of the Sphagnum in like manner is developed as a water-carrying instrument. Its appearance when examined by the microscope, as shown in Fig. 27 and in Fig. 31, is very singular, for it is surrounded not only with large transparent cells of more ordinary shape, but with large cells developed into the shape of flasks, with openings at their tops. Fig. 32 will enable the reader further to realize this structure. It is a highly-magnified section of a quarter of a stem.

Again, the mode of growth of the plant, abandoning its moorings on the soil and throwing out roots into the water, and growing successively year after year, enables it not only to attain great growth, but also, when the occasion demands, to keep pace with the rise of the water in which it may be growing, "the individual thus becoming," it has been said, "in a manner immortal, and supplying a perpetual fund of decomposing vegetable matter."

Fig. 30.—Cluster of cells at base of leaf of *Sphagnum acutifolium*. After Schimper.

Physical Results from Structure.—The result of these peculiarities is that the entire plant of any species of Sphagnum is a perfect sponge. When dry it is capable (as may easily be found by experiment) of rapidly absorbing moisture, and carrying it upwards through the plant; and when growing in vast beds it acts thus on a great scale. Everyone who knows Scotland must know how on many a steep mountain-side, or on the bottom and sides of a gorge, these beds will hold up a great body of water against the force of gravity; and again, the Irish bogs are described as often ascending from the edges towards the interior, sometimes by a gradual and sometimes by a sudden ascent, so that at times the bog is so high that it reaches the height of the church steeples of the adjoining country, without any rising ground intervening.

These peculiarities in the structure of Sphagnum have produced considerable physical effects.

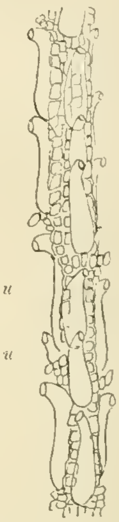


Fig. 31. — Stem of *Sphagnum molluscum*, magnified, showing *u u*, the utricles or flask-shaped cells. After Schimper.

(1) Everyone knows the different effects of rain falling on a land of bare rock or sand, like the Sinaitic desert, and on a porous soil. In the one case it produces a freshet or a flood, that leaves no trace behind: in the other it is held for a while in suspense, and only gradually passes into the streams. The glaciers and the Sphagnum beds of the mountains of Europe alike act as compensation reservoirs—receive large quantities of moisture as it falls, and retain it till the drier season comes, when part of it gradually passes away; but for these reservoirs, many of the rivers would exhibit a far greater shrinkage in summer and autumn than is now the case.

But (2) the Sphagnum beds have become peat, and have gradually filled up the ancient lakes and morasses, and turned water into dry land. It is true that peat appears under some circumstances to be formed by other vegetables than Sphagnum, and in all cases it has probably some other plants or roots growing amongst it. Mr. Darwin tells us that in Terra del Fuego and the Chonos Archipelago, peat is formed by two phanerogamous plants, of which one at least seems endowed with an immortality something like that of the Sphagnum; and the peat of the fens of Lincolnshire is formed mainly of *Hypnum fluitans*. But Sphagnum appears to be the main constituent of peat in Ireland, Scotland, and, so far as my researches have gone, in England; the peculiar spiral threads of the cells of the Sphagnum leaf being easily detected in the peat so long as it retains traces of its organic origin.

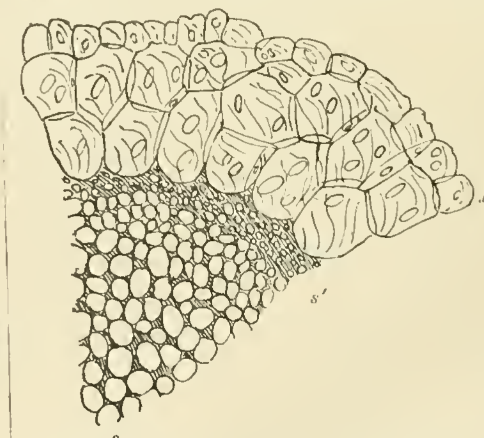


Fig. 32.—Magnified section of stem of *Sphagnum cymbifolium*. $s\ s^1$ stem; $s^1\ x$, mass of spirally threaded cells surrounding stem. After Schimper.

Ancient Forests.—The Peat Mosses, and the sea-shores of our islands and of the adjoining mainland, reveal, as is very well known, traces of ancient forests. Many parts

of England, nearly all the mainland of Scotland, the Hebrides, the Orkneys, and the Shetlands, Ireland, and Denmark, the shores of both sides of the English Channel, Normandy, Brittany, the Channel Islands, and Holland, and the shores of Norway, all bear evidence to the presence of these primeval forests; and what is more, to the successive existence of forests, each in its order living above the buried remains of the earlier ones.

The following table will show the order of succession in the different species of trees in some of the places where this has been observed, the braces representing the co-existence of the trees :—

Island of Lewes.	Danes Moor, near Macclesfield.	Somerset.	Other parts of England.	Parts of Scotland.	Parts of Denmark.
1. Oak.	1. Scotch fir.	1. { Oak.	1. { Oak.	1. { Oak.	1. Scotch fir
2. Elder.	2. Larch.	Ash.	Scotch fir }	Scotch fir }	2. Oak.
3. Birch.	3. Oak.	Yew. }	2. { Birch.	2. Birch.	3. Birch.
4. Scotch fir.	4. Birch.		Hazel. }	3. Hazel.	
	5. Hazel.		3. Alder.	4. Alder.	
	6. Alder.			5. Willow.	
	7. Willow.			6. Ash.	
				7. Juniper.	

In some Irish bogs fir, oak, and yew, and rarely elm, have been found.

What is the cause of the disappearance of these ancient forests one after the other? To this question various answers have been proposed.

The Romans, it has been suggested, in their inroads, cut ways through the forests and laid waste the land. But, wide as was the spread of the wings of the Roman eagle, the phenomenon in question is of far wider extension. They never conquered Denmark, or Norway, or Ireland, or the islands of Scotland: in Scotland, and even in England, their operations could never have covered the whole country; and as regards some of our Peat Mosses, we know that they must have existed long before the Roman invasion; for at least on the borders of Sedgmoor we have traces of their using peat for fuel as it is used there at the present day.

Still humbler agents have been invoked, in the supposition that the beaver and other rodents were the authors of the destruction of the forests. So far as I can judge, the cause suggested seems inadequate to the effect.

Again, changes in climate have been suggested. But, although there may be some evidence from the succession of the trees of a gradual amelioration in the climate, we know of no evidence of changes of so sudden and violent a character as would destroy the existing forests over large areas. Moreover, with few exceptions, the trees of the destroyed forests are such as are now found wild, or will grow easily in the spots where they lie buried.

The overthrow by storms has, again, been suggested as the cause of this wholesale destruction; and the fact that in some of the peat bogs of the west of Scotland the trees that have fallen lie to the north or north-east, and in some of those in Holland to the south-east, in the direction of the prevailing winds in those countries respectively, affords some reason to believe that wind has given the *coup de grâce* to the dying trees, and determined the direction of their fall. But it is much more likely that the work of the wind should be confined to this final overthrow of the decaying trees than that successive forests in full strength should have been swept from the face of vast tracts of Europe by the agency of wind alone. Moreover, in some cases the trunks as well as the bases and roots of the trees are found standing or buried in the bogs.

Allowing that some or all of these agencies may have had their part in the destruction of the forests, I believe that the growth of Sphagnum has been the greatest factor in the work of destruction. "To the chilling effect of the wet bog Mosses in their upward growth must be attributed," says Mr. James Geikie, "the overthrow of by far the greater portion of the buried timber in our peat bogs."

In a letter written by Lord Cromarty, in 1710, on Peat Mosses, and published in the twenty-seventh volume of the *Philosophical Transactions*, we get a curious account of the swallowing up of a forest by a peat bog. In 1651 the Earl saw in the parish of Lochburn (or, as Walker says, at Lock Broom, in West Ross), a plain with fir-trees standing on it, all without bark, and dead. Of the cause of their death he says nothing. Fifteen years after he found the whole place a Peat Moss or "fog," the trees swallowed up, and the moss so deep that in attempting to walk on it he sank in it up to his armpits.

This same process of destruction is still found to be going on in the mountain districts in the Harz and in Thuringia. "Forestry in these highlands," says Graf zu Solms Laubach in his *Fossil Botany*, "is everywhere at strife with the peat bogs, which, left to themselves, are always growing, and by the advance of their margins eat their way into the adjoining forests, and make irregular gaps in them."

(*To be continued.*)

THE LIFE OF AN ANT.—I.

By E. A. Butler.

THE way in which an Ant's nest originates is involved in some obscurity, and it is quite possible that the method is not always the same. Sir John Lubbock has shown that a nest may exist for years without the presence of a female, or "queen" as she would be called, and, on the other hand, if such be present, the number need not, as in the case of bees, be restricted to one. Though a nest which has once been established may continue to exist, at least for a time, without the intervention of any females, still it is most probable that for the origination of a *new* nest we must look to the initiative taken by a queen. The marriage-flight then being over, the young bride, or queen as she may now be called, even though she has no subjects to rule over, has apparently several courses open to her. She might, for example, return to the nest which produced her, or to some other already existent community, and contribute her share towards renewing or enlarging what is already well established. Or again, she might, if of a more independent turn of mind, get some stray workers to help her in founding a new colony of which she would be both the mother and ruler; or, thirdly, there is open to her, supposing her powers equal to the task, the possible

course of taking upon herself all the duties of maternity and colonization, and labouring with her own feet and jaws to prepare a home for, and to support her progeny till they are sufficiently advanced in age to take their proper share in life's burdens. There is scarcely sufficient direct evidence to warrant a decision as to which course would generally be adopted. Sir John Lubbock invariably found that when he introduced queens into queenless nests they were not accepted by the inhabitants, but were at once attacked with energy. Whether this antipathy was owing to a natural tendency, or was the result of the insects in question having been long unaccustomed to the society and rule of queens, is, however, doubtful. And since any eggs that may be laid by workers, a circumstance that sometimes happens, invariably produce males only, it would seem that a nest from which females are permanently excluded must sooner or later suffer extinction. Moreover, as showing that adoption into a strange nest may sometimes be the lot of the newly-hatched queen, McCook records such a case as having come under his own observation in America.

Sir John Lubbock's experiments in the direction of inducing females to rear their own young without help, and so start a new nest, were more successful. One day, in the middle of August, 1876, he found two pairs of the little red stinging Ant, *Myrmica ruginodis*, flying in his garden. He placed them quite apart from other examples of the same species, and provided them with all needful requisites in the shape of earth, food and water. All through the winter they remained alive and healthy—a somewhat unusual circumstance, as the males (Fig. 1) appear generally

Fig. 1.—Male of *Myrmica ruginodis*. Magnified six diameters.

to die in the autumn. In this case they did not die till the spring, just about the time when their partners laid their first eggs. During the course of the summer various eggs were laid at different dates, many of which hatched in due course, passed safely through larvahood and pupahood, and at last reached the perfect state, coming out as workers some thirteen or fourteen weeks after the laying of the eggs. All attentions that they needed during this time were of course rendered by the parent queens, who thus proved that they had the power, if opportunity should call for its exercise, of founding new colonies. Other observers, however, experimenting with different species, have met with less satisfactory results, and it is of course possible that what one species can do in this respect another cannot. The same remark applies to their whole economy; for while the broad facts involved in their social habits are pretty much the same in all, yet no two species are exactly identical in habits, and one must guard against concluding that what is true of one kind is therefore necessarily true of another.

All Ants, of whatever species, commence their life as eggs—these are minute oval bodies, of whitish or yellowish colour. When laid, they have to be stowed away in suitable chambers excavated in the nest, and must be conveyed thither by the workers, by whom also they are carefully guarded. In carrying the eggs the mandibles are used, but to minimize the difficulty of transit, and the risk of damage, which would result from carrying such minute objects singly, they are caused to adhere together by their sticky surfaces, and can thus be conveyed away in batches. It is hardly necessary to point out that what are popularly called "Ants' eggs"—the large yellowish or cream-coloured oval objects which one often finds lying about in great numbers in the passages of the nest on removing the roof, and which are used as food for pheasants, singing birds, and fishes—are not the objects we are now speaking of, but are the pupæ of the Ants, which are looked after by the workers quite as assiduously as the true eggs. Their size alone is sufficient to check any such misconception, and to suggest their true character. But there are often to be found in Ants' nests batches of minute eggs, truly so called, which are not those of the Ants themselves, though guarded with as much care as their own. They are little dark-coloured objects, the eggs of aphides or plant-lice, which are kept by some Ants as domestic animals, and of which we shall have more to say in another paper.

From the egg is hatched, in a few weeks' time, a maggot-like grub (Fig. 2 A), whitish and semi-transparent, and

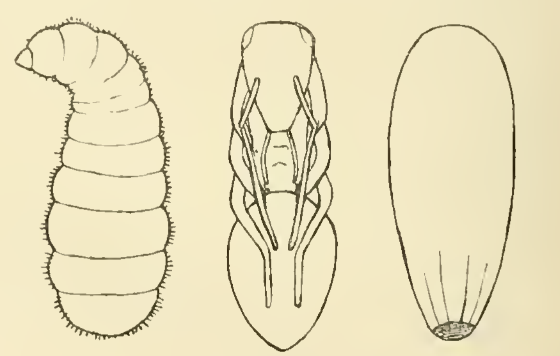


Fig. 2.—A. Larva. B. Pupa. C. Cocoon of Ant.

covered with short hairs; it is plainly divided into segments, but has no legs. It is rather conical in shape, tapering towards one end, at which is situated the mouth, furnished with a couple of tiny hooks, which, though suggestive of jaws, seem to be of little use as such. These larvæ are perfectly helpless, and cannot even feed themselves. Here is one of the penalties attendant upon the development of the social instinct; the insect is never left to itself, but has to be looked after and cared for from the day of its entry into the world as an egg till the time of its extrication from the last skin it will ever shed. In the case of a solitary insect, such as by far the greater number of insects are, no such care would as a rule be taken; the eggs would be left to themselves, and the larvæ would be capable of providing their own food, or if not actually doing so, yet of feeding themselves from a store previously accumulated by the mother. With the hatching of the eggs in an Ant's nest, therefore, the duties of the workers are enormously increased. Scores of little mouths have to be kept supplied with suitable food, each one several times a day, and but a brief intermission of their attentions

would probably be quickly attended with a fatal result. The food is elaborated by the nurses themselves in their own stomachs, and is supplied to the gaping mouths of the larvæ through their own mouths. Now as the eggs are not all laid at one time, there are frequent hatchings going on during the summer months, and in the same nest there will be grubs of different ages; these will of course require differing quantities of food, and possibly even food of varying quality, according to age and to the sex of the insects that are to result from them. Hence method has to be introduced into the management of the commissariat, and the nurslings are divided into classes according to their size.

But the young maggot-like Ant is a troublesome little creature, and needs a good deal more attention than what is involved in providing it with meals. Cleanliness is an important element in its education, and as it cannot clean itself, this duty also falls upon the nurses, which perform the necessary operations with their mouth organs. Just in the same way too the workers will clean one another, and Sir John Lubbock found that the Ants he had marked with spots of paint, so as to be recognizable again, had the spots in a little while removed by their friends, a delicate personal attention which suggests curious ideas of etiquette. For the attainment of the most healthy condition, a certain degree of warmth and moisture is necessary for the larvæ, and, as the various chambers in the nest will usually differ somewhat as regards these conditions, there results the necessity of carrying the babies about from place to place. The mandibles are again called into requisition for this purpose, and, like a cat carrying her kittens, the nurses gently take up the grubs in their jaws, and hurry with them along the galleries from nursery to nursery; if the sun shines, they will be taken to the topmost galleries, not indeed that they may be directly exposed to its rays, for this would apparently be injurious, but that they may be as near to its influence as the thickness of the covering stone or roof of the nest will permit. When the weather is cloudy, or when night comes, they must be carried down; or again, let but the nest be disturbed so as to admit the direct sunlight, and plenty of pairs of eager jaws are ready at a moment's notice to seize and hurry off below ground their fat and fleshy charges, until, in a few minutes, none are left exposed. Of course this may be quite as much for the sake of saving the treasures from the possible grip of the rash intruder, as to remove them from the influence of the direct sunlight.

The length of the larval life varies greatly. One of the common red Ants (*Myrmica ruginodis*) appears to be amongst the quickest in development, and in some of Sir John Lubbock's nests this species remained less than a month in the larval condition. On the other hand, some of the larvæ of the yellow Ant (*Lasius flavus*), viz., those of the autumn brood, are very tardy in their progress, and remain grubs throughout the winter, lying torpid in the deeper parts of the nest, destined not to complete their growth till the spring sunshine re-awakens the population of the nest, and supplies them again with energetic and painstaking attendants. What happens to the grub at the close of larvahood depends upon the species; all change into a chrysalis, but with a remarkable difference, the reason for which is not known. Those which when fully grown have not the power of stinging, as, for example, the common black Ant of the garden, envelope themselves in a silken cocoon, while those which do sting, such as the familiar red species before mentioned, have no such covering, but simply cast the larval skin and remain naked as pupæ. This rule, however, is not absolute, and Latreille discovered that the larvæ of one of our common dark-coloured stingless Ants, *Formica fusca* (Fig. 3) sometimes spin cocoons and sometimes do not. The pupa of an Ant (Fig. 3 B) is similar to that of a bee, but not like that of a moth, inasmuch as it exhibits distinctly the outline of the various parts and appendages of the future insect, such as the head, legs, antennæ, &c. But the whole insect is covered with a thin skin, which has to be removed when it reaches maturity before it can make any use of its perfected limbs. In the case of the stingless Ants then, this insect, enveloped in its thin skin, but with its various parts more or less distinctly revealed, is enclosed in its silken shroud like a mummy in a sarcophagus, the whole constituting a smooth oval body, with a dark dot at the end opposite that at which the head lies (Fig. 2 C). These are the familiar so-called "Ants' eggs" above mentioned, and they have to be carried about from one storey of the nest to another in the same way as the larvæ. In fact, the anxiety of the workers for the welfare of these objects is extraordinary, and those who have kept Ants' nests for observation have made use of this passionate devotion to their young, whether as larvæ or pupæ, to induce the Ants to travel in such directions as may be desired for the purposes of experiment, and the discovery of a store of larvæ or pupæ in any spot is to a worker Ant quite as strong an incentive to exertion, and quite as important a piece of news to be communicated to its fellows, as it would be to come unexpectedly upon an abundant supply of the most delicious food.

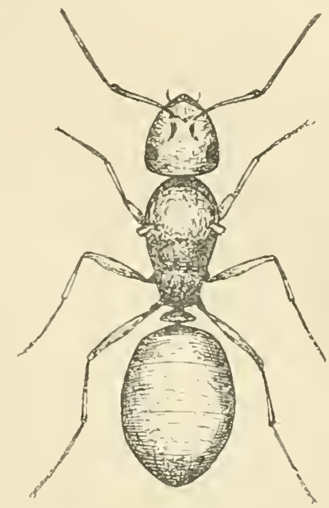


Fig. 3.—Queen of *Formica fusca*, showing remnants of wings.

The change to the chrysalis condition is, of course, not effected by the larvæ till after it has spun its cocoon, for when once it passes into this stage it becomes more helpless than ever before, and would, but for the assistance of the nurses, perish where it is and in direct consequence of its own act of walling itself round, and so cutting itself off from the world by a continuous and impenetrable barrier of silk. Here, again, appears strongly the helplessness of the individual member of the social community, as contrasted with the independence and power of the solitary insect. The caterpillars of many moths, as is well known, before turning into the chrysalis condition, surround themselves with a silken cocoon, which is sometimes, as in the case of the puss-moth, of so hard a consistency as to resist strong pressure, and to offer considerable opposition even to the entrance of the point of a knife. Within this covering lies the limbless pupa, apparently as effectually doomed to a lifelong imprisonment as any captive thrown into the old Bastille. And yet, without any assistance from outside, or any means other than what its own body supplies, the imprisoned moth first throws off its hard and crisp pupa skin, and then works its way through the walls of the cocoon, and after a little pause triumphantly proceeds, in its own unaided strength, to the business of its new life. Not so the Ant, however; for liberty, and indeed, for life itself, it is in many cases beholden to the same unremitting attention that has presided over its destinies hitherto. With a degree of intelligence which is truly remarkable, the nurses divine the right moment when the

imprisoned and invisible Ant is ready for release. Then, with their jaws they nibble and pinch at the loose-fitting investment and make a neat slit down one side, and thus open the prison doors. But even then the captive cannot avail itself of the liberty which is thus brought within its reach. It must be hauled out by the nurses, and stripped of the thin skin which still enshrouds its limbs; those limbs must then be helped into position, and the invalid stroked, caressed and fondled with antennæ and legs till it begins to collect its senses and become conscious of its powers. It must then be led and guided about the nest, till it is familiar with the details of that home in which, if its structure fits it for that purpose, it must now take its place as one of the great band of toilers, and show by its deeds that the care that has been expended upon its education has not been in vain.

Some three or four weeks will have been passed by the Ant in this state of inactivity, during which time it has taken no food. But when it enters on its perfect condition, hunger will again begin to assert itself, though the food taken will not, as heretofore, contribute to swell its bulk, for now waste and repair will be balanced, and the Ant has attained its full size. Amongst the workers there is often a good deal of variation in size; there are large ones which are called workers major, and small ones called workers minor, and often intermediate sizes also; but it must not be assumed that the minors will grow into majors; whatever size the worker Ant has on quitting the pupa skin, that it retains throughout the remainder of its life. The same applies to the males, and to the females as well, except in so far as the development of the eggs increases the size of the abdomen. The range of difference amongst the workers is not the same in all Ants. Take, for example, three of our commonest species; two of these, the yellow Ant (*Lasius flavus*) and the wood Ant (*Formica rufa*) show great inequality in size in the workers, while the common garden Ant (*Lasius niger*) has them much more uniform. The larger workers have often a proportionately larger head, indicating larger muscles and consequently more power in the jaws, whence it has been generally supposed that such forms are intended to act as soldiers and do the chief part of the fighting of the community, but pugnacity is by no means the monopoly of these big-headed forms, and the little workers are quite ready to do battle if occasion requires.

Up to the point to which we have now conducted it, our Ant has passed through a life of considerable monotony. Its most extensive journeys have not extended over more than a few inches of territory, and even these it has performed by the aid of others; its sole occupation, when it has been doing anything more than simply lying still and "developing," has been eating, and for this, too, it has had to be a pensioner on the bounty of friends. It has been from its birth an inhabitant of underground galleries and tunnels, and has never seen the light of day, except by accident. But now a vast change takes place; the whole world is before it, and if it is a worker, there awaits it a life, extending over years it may be, full of variety and activity, and crowded with incident and adventure. It is here that the social insect gets the advantage of the solitary one. The chief businesses that engage the attention of the latter are provisioning and love. The former is often uniform and prosaic enough, and even the latter, while it may involve a certain amount of incident and romance, is still as a rule an affair of such brevity that there is hardly time for anything very striking in the way of adventure to take place, before the adventurer is called upon to pay the debt of Nature. But with the development of the social instinct there comes an indefinite number of new responsibilities and endless opportunities of variety, resulting from the complexity of the life and the increased length of it, which seems to be the necessary accompaniment of the higher type of existence. To the threshold of this life of variety and incident we have now brought our Ant, and there we must leave it for the present, deferring till next month an account of its further adventures.

ELEPHANTS, RECENT AND EXTINCT.

By R. Lydekker, B.A. Cantab.

Assuredly of all the Mammals now inhabiting this earth Elephants are those most justly entitled to the epithet "antediluvian," since they remind us, far more vividly than any of their modern contemporaries, of the gigantic extinct Mammals of various kinds which flourished in that latest epoch of geological history when man was but a comparatively new comer. A long acquaintance has, indeed, made us so familiar with the appearance of Elephants that we are too apt to forget what altogether strange and uncouth creatures they really are. If, however, they had happened to be included among those animals which disappeared from the face of the earth before the historic period, and were known to us solely by their skeletons, there can be no doubt that they would be regarded as among the most remarkable of Mammals. Moreover, if Elephants were only known to us by their skeletons it would be more than doubtful if we should ever have attained a correct idea of their true form; since, although the conformation of their jaws and teeth would clearly indicate that they must have had some very peculiar method of feeding, it would have required a very bold, not to say a very imaginative man to have conceived the idea that these creatures were furnished with that unique organ which we term the trunk or proboscis.

At the present day, it need scarcely be said, there are but two living species of Elephants, differing remarkably from one another not only in external characters, but also, as we shall notice later on, in the structure of their teeth; these two species being respectively confined to Africa, and to India and adjacent regions. These two kinds of Elephants are, however, merely the last survivors of a vast host of extinct forms, some of which were closely related to their living cousins; while others differed so markedly in the structure of their teeth as to have received the distinctive appellation of Mastodons, although they are really nothing but very generalized Elephants. These so-called Mastodons carry us backwards to the middle of that division of the Tertiary period of the earth's history known as the Miocene; but when we have reached to that stage all below is dark as regards the Elephantine pedigree. And it is, indeed, one of the most remarkable circumstances in Palæontology that although we know that Elephants belong to the great group of Hoofed or Ungulate Mammals, of which they form a well-marked division, yet we have practically no sort of knowledge of the many extinct forms which we presume must have connected them with Ungulates of a more ordinary type.

Although the trunk and tusks of Elephants form their most striking external features, yet it is not to these that the naturalist looks at first when enquiring into the true affinities and general structure of these animals, since these come under the category of specialized and acquired structures, which tell but little of an animal's past history; he looks rather to the structure of the internal skeleton,

which is always of especial value as being that part of the organism which is usually alone preserved in a fossil state. Let us then first turn our attention to the skeleton of these animals, of which we may see examples in our larger museums. The most remarkable feature noticeable in such a skeleton

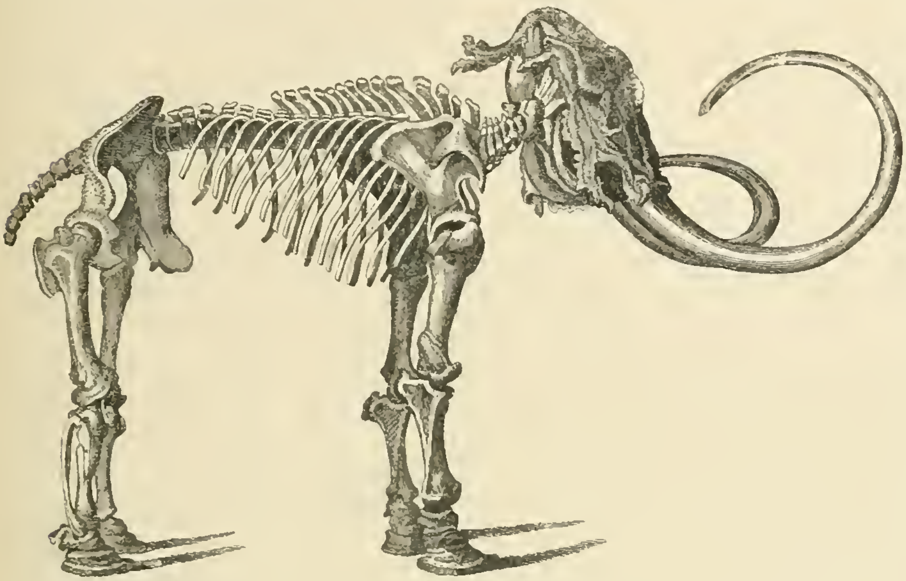


Fig. 1.—Skeleton of the Siberian Mammoth, with the skin still attached to the skull. *(From Jardine.)*

(Fig. 1) is that the various long-bones of the limbs are placed almost directly one above another, so as to form nearly vertical columns of support for the body; whereas in ordinary Ungulates, such as a horse or an ox, these bones are set very obliquely to one another. Moreover, as a similar vertical position of the limb bones occurs in several old extinct Ungulates which are known to be of extremely primitive organization, we may take it that an Elephant's limbs are likewise of a primitive type. We have, however, further evidence in confirmation of this primitive structure. Thus Elephants differ from all other living Ungulates in having five complete toes to all their feet (Fig. 2). Moreover, whereas in other living Ungulates (except the little Hyrax) the bones of the wrist are not situated in vertical rows immediately over the metacarpal bones of the foot, but, on the contrary, cross and overlap one another, in Elephants they have the former relation, with the single exception that the bone marked *l* overlaps the one lettered *td* to a certain extent. This difference may be illustrated by saying that if we were to take a hatchet and chop vertically upwards between the third and fourth toes of an Elephant's foot there would be nothing to resist the passage of the blade till it reached the bones of the leg, whereas in all other Ungulates—the pig, for instance — the blade could not pass through the wrist without cleaving solid bone. Again, whereas ordinary Ungulates walk solely on the tips of their toes, and are thus termed digitigrade, while the bones of the toes themselves are more or less elongated, Elephants walk on the soles of their feet in the so-called plantigrade fashion, and have very short toe bones. Now since all the large extinct Ungulates of the Lower Eocene or earliest Tertiary period also have five-toed feet, very similar to, but still shorter and of even simpler structure than those of Elephants, there can be no doubt as to the extremely primitive plan on which the entire limbs of the latter are constructed. As regards, therefore, its limbs and feet, an Elephant may be said to be an essentially old-fashioned animal.

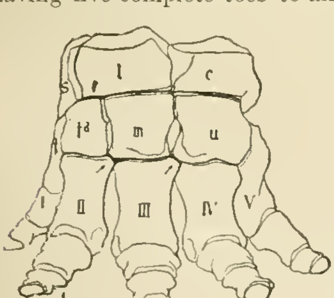


Fig. 2.—Bones of the left fore foot of an Elephant, $\frac{1}{8}$ natural size. The lettered bones are those of the wrist or carpus, and the numbered ones the metacarpals, below which are the bones of the toes.—*(After Osborn.)*

If, however, we turn to their teeth we shall find that Elephants are very far indeed from being of a primitive or old-fashioned type; the truth being that they are, on the contrary, very peculiar and specialized in this respect. The first and most obvious peculiarity in regard to their dentition is to be found in their tusks, which correspond to one of the pairs of upper front teeth in man, and also to the single pair of such teeth in the Rodents (rats, hares, &c). Moreover, these teeth, like the incisors of the Rodents, grow continuously throughout the life of the animal, owing to the circumstance that the pulp-cavity at their base always remains open, and has a permanent connection with the soft structures of the gum. In our own teeth, on the contrary, the pulp-cavity closes at a certain period, after which there is a total cessation of growth. These ever-growing tusks of the Elephant are preceded in the young animal by a pair of small milk-tusks, with a closed pulp-cavity, which are shed at an early period of life. In both of the living species of Elephant the tusks are confined to the upper jaw; but whereas they occur in both sexes in the African Elephant, in the Indian species large permanent tusks are restricted to the male, and are not, indeed, invariably present in all individuals of that sex. The primitive Elephants, or Mastodons, frequently, however, had tusks in both the upper and lower jaws; and since these did not generally attain the huge dimensions which they reach in many true Elephants, it is evident that in this respect the Mastodons departed less from the ordinary type of Mammals, where the front teeth are not greatly larger than the hinder ones, and those of the upper and lower jaws correspond with one another in size and number. Before leaving the subject of tusks, it may be mentioned that the ivory of which they are composed differs from the so-called ivory of other teeth in a manner which renders it always easy to determine whether a reputed ivory article is genuine. This peculiarity consists in the circumstance that a transverse section of a tusk exhibits a series of fine, decussating, curved lines radiating from the centre to the circumference, and forming curvilinear lozenges at their intersections. This peculiar structure is in fact precisely similar to the "engine-turning" on the back of a watch; and in an ivory knife-handle it should be distinctly visible at the butt. *(To be continued.)*

THE MOVEMENTS OF THE STARS.

By Miss A. M. Clerke.

Proper motions may be described as the individual, apparent, angular displacements of the stars. They are small residual irregularities, becoming persistently manifest year by year, or century by century, after all the usual systematic corrections have been applied, through which star-places of various dates are rendered strictly comparable. And because they are individual, they can only be ascertained by observation. No process of reasoning is available by which, from the known movements of nine hundred and ninety-nine stars, the unknown movement of a thousandth star can be calculated. There may be a logic by which the feat could be accomplished, but we are absolutely ignorant of its rules. Proper motions are then experiential data, defying formulaic expression, which have to be tabulated one by one—each as a fact apart. Some thousands of them are now, with very fair accuracy, disposable by astronomers; and their registration has been no trifling achievement. Yet they are only the unshaped stones out of which the edifice of knowledge regarding sidereal structure has to be built: or say rather, the pile of clay collected antecedently to the moulding and baking of bricks for that high purpose.

It is true that mere empirical acquaintance with proper motions serves all the purposes of practical astronomy, since it confers the power of predicting, for an indefinite time to come, the places on the sphere of the stars they affect. But the physical astronomer has other ends in view. For him the "sphere" has no more reality than the crystalline barriers fencing off one from the other the successive heavens of the ancients; he regards it as an ideal canvas, upon which the signs and wonders of the sky are pictured. His true concern is with the ocean of space, and the voyages amid its depths of the fiery craft everywhere furrowing them.

The direction and speed, however, of these voyages are only in part and imperfectly indicated by telescopically measured proper motions. No determinate results, in the physical sense, can be elicited from them until they have first been combined with further items of information of three several kinds. For they include, to begin with, a common perspective element due to the translation of the solar system. This must be eliminated as a preliminary to assigning the "peculiar" movement due to each star as a body traversing space on its own account. Secondly, proper motions are angular amounts only. To render them linear, we should be acquainted with the distances from ourselves of the objects displaced by them. Thirdly, proper motions are the projections upon an imaginary plane of lines of travel forming unknown angles with that plane. Only that portion of stellar movement which lies across the visual ray is represented in them; they tell nothing about the component along the visual ray.

None of these three requisites for deducing individual real motions from proper motions is indeed entirely wanting even now; and all are likely to be more completely provided in the future. A thoroughly satisfactory determination of the sun's course and velocity will evidently before long be accomplished through the accumulation of precisely known stellar radial movements; and by the same means, the missing component of proper motions first cleared of the effects of our own interstellar journey, can already be supplied in some cases, and will before long, be supplied in many more. The list of measured star-parallaxes, too, lengthens continually; yet here it must be admitted that the prospect of improvement is less assured than elsewhere. For relative parallaxes only are given by the methods at present in use, and doubts of the gravest kind as to the validity of conclusions from relative to absolute parallax are beginning to force themselves upon astronomical attention. It is, nevertheless, consolatory to reflect that misgivings as to the genuineness of the results so far attained in this important branch of research are strongest where sensibly fixed objects are in question. Stars with appreciable proper motions detach themselves from their background, and so afford a satisfactory criterion for the choice of trustworthy reference-stars. Hence the parallaxes of swift stars can rarely be wholly illusory, and are probably, for the most part, but very slightly underestimated.

In the meantime, what conclusions can be derived from the facts actually before us? Do they provide ground for even a rational surmise as to the dynamical relations of the stars? At the threshold of the enquiry we are startled by the phenomena of what are called "runaway stars." Of these, Groombridge 1830, with its thwartwise velocity of 232 miles a second, is the classical example; but it is outdone by Arcturus with 375, and by μ Cassiopeiæ with 305 miles a second; and several stars besides—if the small parallaxes attributed to them can be depended upon—shoot through space at rates varying from 70 to upwards of 100 miles a second. Twelve, moreover, out of 52 stars with numerically valued tangential velocities, progress at a speed exceeding 50 miles a second. Stars, then, of the "flying" description, are no great rarities. Among them are to be found both single and compound objects, enormous bodies like Arcturus, and orbs on the modest solar scale, such as Groombridge 1830; and they show spectra of sundry varieties. Some are probably highly luminous in proportion to their mass, others give little light, while exercising a strong mutual attraction. Of the latter sort, at least, are the semi-obscure revolving pair carried with it by o^2 Eridani in its particularly well authenticated advance of 71 miles a second. The swiftest stars cannot, accordingly, be regarded as forming a class by themselves; their velocities, although unaccountable, and, by calculable gravitational power, uncontrollable, are evidently systematic, that is, belong to the settled order of sidereal arrangements, and have to be dealt with in any attempt to grapple with the problem of sidereal mechanism.

No radial velocities at all comparable with the high tangential velocities of late pretty freely disclosed, have yet been detected. Aldebaran, with 30 miles a second of recession from the sun, bears the palm among 47 stars spectrographically determined at Potsdam, and their average rate comes out no more than 10·6 English miles. But the average value of the visible component for 52 stars with ostensible parallaxes,* is 42 miles, the effects of the solar motion being of course impartially mixed up with both kinds of result. The tangential rate, however, since it is measured along great circles of the sphere, sums up motion in the two co-ordinates of right ascension and declination, the separate value of each of which, amounting to just 30 miles a second, is the quantity properly to be contrasted with the figure derived from the Potsdam observations.

A striking disparity, none the less, still remains, and suggests curious reflections; although no great stress can be laid upon them until it be seen whether the anomaly giving rise to them be abolished or accentuated by further research. Possibly it may be due to the character of the objects examined for two different purposes. The Potsdam

* One of them is Arcturus, which can scarcely be said to have even an "ostensible" parallax, since its probable error considerably exceeds its nominal amount. The star should rather be called indefinitely remote.

list comprises mainly stars of the second magnitude, which, for some unknown reason, possess exceptionally slow visible motions; and it excludes those faintly-luminous, swift objects so frequently chosen as the subjects of experiments for parallax. Hence, when spectrographic measurements are extended to stars of the sixth and seventh magnitudes, radial motions of hundreds of miles a second may be brought to light. There seems no good reason why they should not exist: for in our present ignorance as to the dynamical relations of the stellar crowd, we cannot with any confidence anticipate the display by its members of preferences in the shaping of their courses. Arcturus, then, which chances to have nearly the whole of its tremendous speed directed *across* the line of sight, may prove to have opposite counterparts in stars telescopically fixed, but rushing with scarcely less rapid *end-on* motions towards, or away from the earth. The discovery, however, of such hypothetical objects will demand perseverance or good fortune; since no suspicion can be raised beforehand of a peculiarity betrayed by no visible symptom.

If indeed there were reason to believe the stars combined into a stable system, and wheeling unanimously round a common centre, then their tangential might be expected to prevail very greatly over their radial velocities, as measured from a point anywhere in the neighbourhood of that centre. Thus, the planetary movements would lie nearly altogether across the line of sight of an observer on Mercury; and the movements in the Saturnian system would be similarly related to one stationed on Mimas. But the plan of stellar organization bears no traceable resemblance to that followed in the planetary and Saturnian systems; so the argument from analogy falls to the ground. Still, it is important to recall that the postulate of approximate equality between the average values of different components of stellar velocity, besides being flatly contradicted by our present, certainly most imperfect, experience, really involves some kind of theory, or negation of a theory, regarding sidereal construction.

This postulate has lately served for the foundation of an attempt to solve the problem of mean stellar distance. Dr. Kleiber (*Astr. Nach.*, No. 3037) laid it down as a principle not likely to be disputed, that, in a sufficient number of casually distributed velocities, the sum of movement projected in any given direction cannot differ very appreciably from the sum of movement projected in any other; and proceeded to compare the radial speed of 22 stars measured by Professor Vogel with their proper motions as determined by Dr. Auwers. The sum of these last proved to be, in right ascension 4·83″ (after due multiplication by the cosine of the declination), in declination 5·51″; while the sum of the corresponding end-on movements was 363 kilometres per second. Hence, taking an arc of 4·83″ to stand for so many kilometres as result from multiplying 363 by the number of seconds in a year (31,556,929), the mean parallax of the 22 stars comes out 0·065″, implying a light journey of 50 years; the proper movements in declination giving similarly a parallax of 0·074″ (light journey = 44 years). The mean magnitude of the stars made the subjects of this interesting, if not entirely convincing, experiment is 1·8; so that its upshot is in plausible agreement with Dr. Elkin's directly measured mean parallax of 0·089″ for 10 stars of the first magnitude.

The real significance of stellar movements has yet to be penetrated. Setting aside the accordant impress stamped upon them by the sun's onward march, they seem almost purely erratic. Certainly they obey no obvious stream of tendency. They are executed indifferently in all planes, and show no methodical graduation of velocities. They disregard the lie of the Milky Way, and refuse to allow a "rotation-component" with reference to it to be elicited from them. Shall we then give ear to a "counsel of despair," and assert that the "mighty maze" is "without a plan"? This would surely be rash; despair always is rash. Millenniums indeed are but as moments in the development of the harmonizing idea which we expect to lay hold of and apprehend in the course of a few generations of ephemeral existence. But that such an idea is present—that the stars do not cruise at random and rudderless, drifting at the mercy of wind and tide, so to speak, through space—a very little consideration suffices to show.

The case, on a general view, seems to stand thus. The recorded observations of stellar movements all, or nearly all—for radial measurements are independent of distance—refer to stars comparatively near the sun. These are promiscuously distributed throughout the vast region enclosed by the annulus of the Milky Way, yet after a manner not wholly irrespective of its structure. They show, on the contrary, decided condensation towards the plane of that stupendous collection, and form with it, undoubtedly, one highly complex aggregation. No aggregation of moving bodies can, however, continue to exist apart from their subjection to some governing law. "Bees in a swarm," for instance—to adopt Professor Young's chosen example—would very quickly cease from that condition unless maintained in it by an efficacious "clustering power." Their courses must be more or less pendulous, if not centrally deflected. A few seconds of persistent flight along straight lines would bring about the disintegration of the little community. And so, just because the sidereal aggregation is a fundamental reality, and not the casual product of an eternal and meaningless flux of things, star-movements must be controlled by some primary and overmastering force, acting uniformly, universally, and irresistibly. Did they truly possess the lawless and insubordinate character sometimes imputed to them, we should be compelled to regard the constellated suns of our midnight skies as mere adventurers from the void brought into temporary companionship within the circuit of the galaxy, and obeying only King Arthur's law of life—

"From the great deep to the great deep they go."

But this is simply incredible.

[A general aggregation of the brighter stars towards the zone of the Milky Way has long been recognized. Dr. Gould has carefully studied the symmetry in the arrangement of the brighter stars, and points out (in the *Uranometria Argentina*, p. 361), that the brighter stars are more uniformly distributed with respect to the medial plane of the zone of great stars (described in the article on the Pleiades cluster in Knowledge for May, 1891) than with respect to the medial plane of the Milky Way. The zone of large stars follows a great circle round the heavens, making an angle of about 20° with the medial plane of the Milky Way.

It does not follow, from the general symmetry in the arrangement of the brighter stars which is observable, that all the brighter stars belong to the Galactic system, or that they are connected with the great zone above referred to. A third or even a half of their number may be distributed at random over the sphere. The chance distribution of a certain number of stars would not obliterate any symmetry that might be evident in the arrangement of the stars of the Galactic cluster.

Some twelve years ago Professor Simon Newcomb showed that stars having a velocity in space of 25 miles a second could not be controlled by the gravitation of the Milky Way system, and that such swift stars could not therefore be

permanently associated with it. The assumptions made use of by Professor Simon Newcomb are probably far too liberal (see *Old and New Astronomy*, part XII.). Our own sun and even stars having a velocity of 15 miles a second must probably be assumed not to belong to the Galactic system. It is a curious fact that stars with the solar type of spectrum have on the average large proper motions compared with stars giving the Sirian type of spectrum, and that Sirian stars cluster thickly towards the Milky Way and the great star zone—while Solar stars seem to be much more irregularly distributed. It points to the conclusion that the Galactic system is a cluster of Sirian stars associated with nebulæ through which the sun happens at present to be passing.—A. C. Ranyard.]

THE MOON'S ATMOSPHERE.

By A. C. Ranyard.

If the Moon had a copious atmosphere at all like our own the parts of the Moon near its limb or outer smooth edge would appear reddish and decidedly less bright than the central portions, for the limb of the Moon would be seen through a great depth of the lunar atmosphere, and the light of the limb would be reduced just as the light of our sun is reduced at rising or setting as compared with the light of the mid-day sun. But the limb of the Moon is not in the least dimmed or hazy. In fact, it is always one of the brightest parts of the Moon's disc, and is very sharply defined, as will be seen by examining the plate published with this number, which is made from one of the beautiful photographs of the Moon taken by the Brothers Henry, and if the Moon's atmosphere were at all like our own we should expect to see a sensible zone of twilight along the Moon's rough edge, but the shadows between the illuminated mountain tops appear on the photographs and to the eye perfectly black. There is no recognizable refraction when the Moon comes between us and a more distant object, such as the sun, during an eclipse, when we should expect an atmosphere to cause a very evident distortion of the sun's limb where the Moon cuts it, and there is no recognizable prolongation of the cups of the thin solar crescent into a narrow ring of light such as was observable round Venus just before it entered upon the sun's limb.

But several observers of partial eclipses of the sun have described a band or shade on the sun just outside the Moon's limb. It cannot amount to a very notable darkening of the sun's disc, for all observers do not notice it; some have looked for it and have not been able to detect anything of the kind. On the other hand, many photographs of the partial phases show a bright band on the sun's surface running along the outside of the dark Moon's disc. This brighter band on partial-phase eclipse photographs was at first thought to be due to an effect of contrast, but its actual existence as a band of denser photographic action on photographs taken during the eclipse of August 7, 1869, in America, was demonstrated by Dr. Edward Curtis, who showed that the photographic action produced by the sun's light was denser close to the Moon's limb than at a little distance from the limb; so that printed dots placed behind the negatives were hidden by the brighter band adjacent to the Moon's limb, while they could be distinctly seen through the photograph of the sun's disc at a little greater distance from the Moon's limb.

Photographic plates often show differences of brightness of different parts of the sun's disc which are not recognizable by the eye; thus a not too much exposed photograph of the whole sun's disc generally appears much fainter towards the limb, while the eye hardly notices the greater brightness of the sun's central parts as compared with those near the edge of the disc. A band of *increased* density might, it is suggested, be caused in an over-exposed photograph by a part of the sun's disc being dimmed down by a lunar atmosphere, so that the well-known reversing action (which after a certain limit of exposure begins to take effect) goes further on the brighter regions of the sun's disc, away from the lunar limb, than on the region close to the lunar limb.

The earth's shadow through which the Moon passes during a lunar eclipse is always found to have a slightly greater diameter than the geometrical shadow calculated from the known diameters of the sun and earth. The excess of the diameter of the observed shadow region over the calculated diameter of the geometrical shadow is generally put down at about one-sixtieth, that is, the earth casts a shadow as if its atmosphere to a height of about 66 miles above the sea-level acted as an opaque rind or covering through which the sun's light cannot pass without having its intensity materially diminished.

The Earl of Rosse and Dr. Boeddicker, in their account of observations of the Moon's radiant heat* observed during a total lunar eclipse, came to a somewhat similar conclusion with regard to the heat-absorbing power of the earth's atmosphere, which they believe is recognizable with their great 3-foot telescope and apparatus to a height of not less than 190 miles above the earth's surface.

The measures of the diameter of the shadow of the earth through which the Moon passes during a lunar eclipse are necessarily very indefinite, but we may probably assume that the Moon has not an atmosphere which corresponds in light-absorbing properties to the earth's atmosphere at a height of 50 miles above the sea-level, that is (according to the law of decrease of density stated in the article on the Earth's Atmosphere in Knowledge for November last), the Moon probably has not an atmosphere which is $\frac{1}{20000}$th part as dense as our own atmosphere at the sea-level. But our atmosphere, if it were transferred to the Moon, would only be about one-sixth as dense at the Moon's surface as it is at the earth's surface, for gravity at the Moon's surface is only about one sixth as intense as terrestrial gravity at the earth's surface, and though the atmosphere would continue to support about 30 inches of mercury at the lunar surface, both the mercury and the atmosphere would have their weight reduced to one sixth of their terrestrial weights. Consequently the atmosphere at the lunar surface being compressed by only one sixth of the weight which it is compressed by here would occupy six times the volume it occupies here, and it would be one sixth as dense as the atmosphere at the earth's surface, and neglecting differences of temperature, the whole lunar atmosphere would extend six times as high as the earth's atmosphere—so that if, in pursuance of the reasoning of the article in the November number, we assume that the earth's atmosphere does not extend to a height of 250 miles above the earth's surface, the lunar atmosphere would not extend to a height of 1500 miles above the lunar surface.

Let us now follow the Moon backwards in time according to Prof. Geo. Darwin's assumption, until it approached Roche's limit, where it would be torn to pieces by the tide produced in it by the earth's attraction and broken up into a ring, say (assuming the

* Published in the Scientific Transactions of the Royal Dublin Society, 1891.

South.

West.

East.

PHOTOGRAPH OF THE MOON.

Taken by MM. Paul and Prosper Henry with their 13-inch refractor, at the Paris Observatory. The sensitive plate was placed behind an eye-piece which enlarged the image in the principal focus 15 diameters. Age of the Moon 167 hours. Taken March 27th, 1890.

The Automatic Engraving Company, Strode Road, Willesden Green.

earth and Moon to have their present diameters and to be homogeneous bodies) when the Moon was at a distance of 10,000 miles from the earth's centre.

Until Roche's limit was approached the summits of the terrestrial and lunar atmospheres would be widely separated by a gulf of more than 3000 miles, and unless one or both bodies were very hot, and the heights of their atmospheres were greatly raised by temperature, we cannot suppose that there would be any intermixture of the atmospheres and consequent drafting of gas from one body to the other. If, however, they were both in a sun-like condition and their atmospheres or coronas intermixed, we should have an exchange of gaseous matter going on.

If they were both about the same temperature, the smaller body having the least gravitating power, would have the highest atmosphere, and a transfer of gaseous matter would go on from the smaller body to the larger body. If, however, the smaller body cooled first and the larger remained in the sun-like condition, we might have a transfer of gaseous matter from the larger to the smaller body, owing to a condensation of vapour on the side of the smaller body remote from the larger. Such exchanges may possibly account for the different density of the earth and the Moon,* and also, as I have suggested in another place, for the different colours of binary stars, and the curious fact that the smaller star of a binary is apparently always bluer than the larger star.

* The earth's density being taken as unity the Moon's density is 0·613.

STELLAR SPECTRA.

To the Editor of Knowledge.

Sir,—In Knowledge for June, 1891, Mr. Maunder announced, for I believe the first time, a principle as to the relative brightness of stars with different spectra, which seems destined to bear very important fruits in Astronomy. I have lately compared the stars treated of by him with the Draper Catalogue of the Harvard University, with the result of removing most of the anomalies in his table, save that of the extraordinary brilliancy of γ Leonis. Four stars with spectra of the first type (Sirians) in Mr. Maunder's list, have a relative brightness of less than 4·0. These are O Σ 4, relative brightness 1·48, ξ Cancri, relative brightness 2·90, η Coronæ Borealis, relative brightness 1·40, and μ^2 Bootis, relative brightness 2·74. Mr. Gore (whose figures Mr. Maunder follows) seems to have under-rated the magnitude of the first of these stars. Its photographic magnitude is 6·46 according to the Draper Catalogue, if I am right in my identification, and I find that even with Sirian stars the photographic magnitude is usually less than the photometric. Taking its magnitude at 6·46 its relative brightness is 5·53 instead of 1·48. Otherwise it is not in the Catalogue. ξ Cancri, according to the Draper Catalogue, has a spectrum of the Solar not the Sirian type, and the same remark applies to η Coronæ Borealis. μ^2 Bootis is not in the catalogue at all. The star with a spectrum of the first type is μ Bootis, a brighter star which is situated near the binary pair, and I suspect that it was the spectrum of this star that Mr. Gore inserted in his table. 4·00 thus seems to be the minimum of relative brightness for a Sirian star, if indeed any of them sink so low, while there are only five solar stars in Mr. Maunder's table (besides γ Leonis) which rise so high. The brightest of these is π Cephei 11·07, the spectrum of which is marked with a ? in the Draper Catalogue, and from a note it appears that on special examination the spectrum was considered to be of the *third* type. That stars of this type should be relatively brighter than those of the second type (Solar stars) is no doubt an unexpected result, but it is confirmed by the next case, 36 Andromedæ 6·23, whose spectrum is also referred to the third type in the Draper Catalogue. τ Ophiuchi 7·35 is the brightest of the remainder, Doberck's orbit being adopted. As different computers have differed considerably with regard to the period of this star, I think the orbit would be worth re-computing, including recent measurements. The relative brightness of the remaining two is under 5·00. I may note that several stars classed by Mr. Gore and Mr. Maunder as Sirian, appear as Solar stars (spectrum F or E) in the Draper Catalogue. Their spectra seem to lie on the border-land, and Pickering gives a wider extension to the second type of spectra than to the first. But these stars confirm the general conclusion drawn by Mr. Maunder, for their relative brightness is in all cases below the Sirian average and above the Solar average. Their intermediate position is thus established.

I remain, your obedient Servant,

January 5th, 1892. W. H. S. Monck.

P.S.—I find that I was in error in stating that the photographic magnitudes are usually less than the photometric in the case of stars with spectra of the first type. With the brighter stars they are almost invariably less, but with the fainter stars they are often greater. It is, in fact, clear that the two scales do not proceed on a common basis. W. H. S. M.

THE INHERENT LIGHT OF JUPITER.

To the Editor of Knowledge.

Sir,—Among the "other considerations" referred to by Mr. Gore in his very interesting article (on page 36), whence the self-luminosity of Jupiter may be inferred, perhaps I may be allowed to recall those derivable from an observation of my own made in 1880, of which the details will be found on page 47 of vol. XLI. of the R.A.S. *Monthly Notices.* On the occasion to which I refer the shadow of Satellite II. was distinctly of a chocolate colour; and the only feasible explanation that I could find of this phenomenon was, that the portion of the planet upon which the shadow was projected must have been red-hot. If we were to illuminate a whitewashed wall by light from a lamp or lantern transmitted through a red glass, obviously the wall would appear red. If now, we flooded it with the electric light, this would wholly overpower the more feeble illumination, and the wall would appear white. It is evident, though, that any object interposed between the electric light and the wall would cut off the more brilliant form of illumination, leaving the feebler one to assert itself; and that hence shadows would appear not black but red. I may add that Mr. Campbell, F.R.A.S., observed this brown shadow of Jupiter's second satellite at Barnet, simultaneously with my own observation of it in Sussex.

I am, faithfully yours,

Forest Lodge, Maresfield, Sussex, William Noble

February 2nd, 1892.

[The intensity of the inherent light of Jupiter must be small as compared with the brightness of the sun's light at the distance of Jupiter, for the satellites are entirely lost to our view when they plunge into the shadow of Jupiter. If their brightness were reduced only eight stellar magnitudes when they plunged into the shadow of the planet they would still easily be visible in large telescopes. For the third satellite, which is the brightest, is usually estimated as equivalent in brightness to a star of the sixth magnitude. Therefore, we may assert that the illumination derived from the whole disc of Jupiter (which subtends an angle of nearly 19°, as seen from the nearest

satellite) is not one fifteen-hundredth part as great as their solar illumination, or the illumination derived from the sun, which subtends an angle of less than 6′ as seen from the distance of Jupiter's satellites, must be more than 1500 times as great as that derived from the intrinsic luminosity of the planet. I am therefore inclined to think that Capt. Noble's observation must probably be explained as due to contrast or some other physiological cause. I have been frequently struck by the amount of colour visible on the disc of Jupiter as seen in an 18-inch silver-on-glass reflector. To my eye the body of Jupiter shows warm tints of red, canary colour, and blue, which are very noticeable directly the attention is directed to the colouring.—A. C. RANYARD.]

[Having been kindly favoured by the editor with an opportunity of reading the above note, I would merely say that the tints of which he speaks on Jupiter have their ultimate origin in sunlight, as much as the red colour of a holly berry or the yellow of a primrose has; and that, hence, such tints would be wholly invisible were the sunlight cut off from the surface emitting them by the shadow of a satellite. I perhaps might have added to my original letter, that, during the visibility of the chocolate-coloured shadow of Satellite II., that of Satellite I. was seen on another part of the planet's disc, like a small circular spot of the blackest ink. The fact that Mr. Campbell simultaneously saw (absolutely independently) the shadow of the second satellite of the same colour as I did, surely negatives any subjective cause of the phenomenon. A physiological cause must, so to speak, be idiosyncratic.—W. NOBLE.]

To the Editor of KNOWLEDGE.

SIR,—Having, during the past six years, devoted my Observatory to the investigation of the light curves of variable stars, I venture to draw the attention of your readers to the following phenomena which I have not seen noticed elsewhere, except in the case of the Nova Aurigæ, which was described in *The Times*, 3rd inst., as "slightly fuzzy" when first observed:—

I have given below extracts from my note book with regard to three stars, and have, with my assistant, Mr. Grover, frequently observed—

(*a*) A remarkably well-defined, almost planetary, disc.

(*b*) A well-defined star, surrounded by a more or less dense, ruddy atmosphere.

(*c*) A large, woolly, ill-defined image, resembling a small but bright planetary nebula.

(*d*) At minimum, in place of the variable, a slight bluish nebulosity.

I do not consider that I have a sufficient series of observations on which to base any theory, but trust that other observers may be induced to take up what appears to me to be an important field of enquiry. The telescope in use is an achromatic of $6\frac{1}{4}$ inches aperture.

Yours faithfully,
CUTHBERT E. PEEK.

Rousdon Observatory, Devon,
February 17th, 1892.

T. Cassiopeiæ. 1889, April 29th. 7·9 mag. Very red, surrounded by a ruddy haze in striking contrast to the clear white of No. 6 (another star in the field). September 10th. 7·7 mag. Very red. A well-defined star shining through a ruddy haze ("a" night; moonlight). September 18th. 7·9 mag. Very red and hazy. ("a"* night). 1890, March 9th. 9·9 mag. Very deep red, ill-defined. ("a" night.) October 16th. 6·6 mag. Large and ill-defined. ("a" night.)

R Cassiopeiæ. 1886, September 17th. 8·7 mag. A large ill-defined deep red star. 1887, February 16th. 9·5 mag. Very red, and not so sharply defined as the other stars in the field. March 13th. 8·7 mag. Very red. Cannot be focussed clear and sharp. September 19th. 7·6 mag. There is great difficulty in getting a good image. The star seems as if surrounded by a dense atmosphere. October 24th. 7·2 mag. With power 136 it seems surrounded by an atmosphere, or as if shining through fog. 1889, March 11th. 9·6 mag. Very hazy and ill-defined as if shining through ruddy mist. Other stars sharp. July 21st. 8·9 mag. Extremely deep ruddy. Ill-defined and hazy, quite unlike surrounding stars. ("a" night.)

S. Herculis. 1891, September 27th. 13 mag. A very minute point. October 14th. No star visible with powers 80 or 34. With 132 a faint nebulosity is suspected in the place of the variable; on slightly swaying the telescope a bluish nebulosity is certainly seen. October 28th. The nebulosity is best seen with power 136. The minute stars near the place of the variable are well seen, clear and sharp as usual. November 25th. The variable has reappeared as a minute but sharply-defined star.

* NOTE.—The letter "a" means a night of excellent definition. Other stars with regard to which similar remarks appear in my note-book, S. Cassiopeiæ, R. Tauri, R. Aurigæ, V. Cancri, R. Ursæ Majoris, S. Ursæ Majoris, R. Camelopardi, R. Bootis, S. Coronæ, R. Aquilæ, S. Cephei.

[It is difficult to conceive of physical changes taking place rapidly on the vast scale indicated by Mr. Peek and Mr. Grover's observations. Possibly the blue nebulous haze or corona round these variable stars may only become visible to us when their light is small—as the variable star increases in brightness it seems possible that the light of the star diffused in our own atmosphere may blot out or eclipse the faint light of the nebula surrounding the variable.

During a total eclipse the light of the solar corona becomes visible through the illuminated atmospheric veil for the last few minutes before the "totality." If we could suppose our sun to be a variable star, and its light to be diminished till it equalled that of the thin crescent of photosphere left visible a few minutes before totality, we should see the sun under ordinary daylight conditions as surrounded by the corona, but as the light of the solar disc increased, the corona would be hidden by the solar light, diffused by our atmosphere, without any change necessarily taking place in the brightness of the corona.—A. C. RANYARD.]

THE CAUSE OF THE ICE AGE.

To the Editor of KNOWLEDGE.

SIR,—Since my letter on the cause of the Ice Age I have seen Sir R. Ball's book on the subject, in which he cites the passage (from 368*c* of the *Outlines*) on which he relies as establishing Herschel's inaccuracy. It is, no doubt, carelessly worded, and goes some way towards justifying the criticism, but I think the context shows that it was at worst a slip of the pen. Sir John Herschel first gives the usual explanation of the seasons, in which it is obviously implied that each hemisphere receives less heat during its winter than during its summer; then he proceeds (368*a*), "Let us now consider how these phenomena are *modified* by the ellipticity of the earth's orbit," &c.; and in considering the modification, he does not again refer to this unequal distribution of heat. In 368*c*, however, he says that at the period of greatest eccentricity if the perihelion took place at midwinter (for the northern hemisphere) "the state of the two hemispheres would be strongly contrasted. In the northern we should have a short but very mild winter with a long but very cool summer, *i.e.*, an approach to perpetual spring; while the southern hemisphere would be inconvenienced and might be rendered uninhabitable by

the fierce extremes caused by concentrating *half* the annual supply of heat into a summer of very short duration, and spreading *the other half* over a long and dreary winter, &c." The language is, no doubt, incautious, but I think Sir John Herschel should be reasonably understood as speaking of half the supply of heat received, not by each hemisphere, but by the earth as a whole. Otherwise there would be no such contrast. Whether *called* winter or summer, each hemisphere would have a short hot season and a long cold one—each hemisphere receiving one-half of its heat in 166 days and the other half in 199. Moreover, the equable distribution—"the approach to perpetual spring"—would occur when the eccentricity was not at a maximum but at a minimum, and if the orbit became circular there would be no seasons at all. At all events, the erroneous statement forms no part of Herschel's general theory, which proceeds on the assumption that each hemisphere receives less heat during its winter than during its summer, owing to the obliquity of the ecliptic; and I cannot find that the inaccurate language which I have cited misled Croll.

10th February, 1892. W. H. S. Monck.

A CURIOUSLY ERRONEOUS STATEMENT.

To the Editor of Knowledge.

Sir,—In your January number Sir Robert Ball attributes to Sir John Herschel a statement about the solar heat received by the earth, "to the effect that the heat was equally distributed, so that fifty parts were received in summer and fifty parts in winter." He says besides, that "Croll and other writers adopted Herschel's mistake as a scientific verity, and reared on this untrustworthy foundation no small superstructure of figures and arguments."

These grave charges against Sir John Herschel and Dr. Croll previously appeared, perhaps notwithstanding some friendly remonstrance, in a little book published by Sir Robert Ball under the title of *The Cause of an Ice Age*, where we are told (twice upon one page, 89) that this amazing assertion of equality has often been made; although the book itself (page 128) speaks of "the familiar difference between the mean temperature of summer and that of winter," adding that we may well be surprised "when we notice that the contrast in warmth between the two seasons is not a much larger one than our experience has shown it to be."

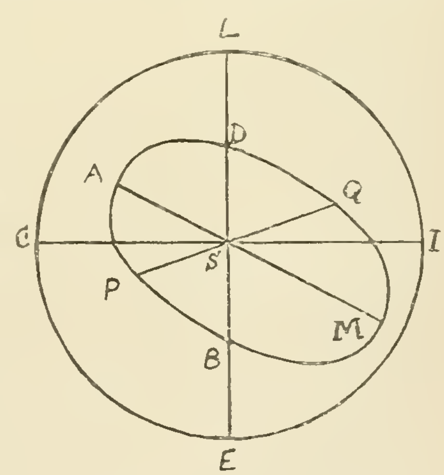
Yet Sir Robert Ball is so sure this mistake was actually made by Sir John Herschel, and often accepted by others, that its correction seems to be given (chapter vi.) as the principal reason why his book has been written.

In Herschel's *Outlines* (Articles 336, 337), a few pages before the passage cited by Sir Robert Ball in proof of his charge, that illustrious astronomer says (referring to a diagram in which A is the vernal equinox, B the summer solstice, C the autumnal equinox, and D the winter solstice), "Whenever, then, the sun remains more than twelve hours above the horizon of any place, and less beneath, the general temperature of that place will be above the average; when the reverse, below. As the earth, then, moves from A to B, the days growing longer, and the nights shorter, in the northern hemisphere, the temperature of every part of that hemisphere increases, and we pass from spring to summer; while at the same time the reverse obtains in the southern hemisphere. As the earth passes from B to C, the days and nights again approach to equality—the excess of temperature in the northern hemisphere above the mean state grows less, as well as its defect in the southern; and at the autumnal equinox C the mean state round again to A, all the same phenomena, it is obvious, must again occur, but reversed—it being now winter in the northern, and summer in the southern hemisphere. All this is exactly consonant to observed fact. The continual day within the polar circles in summer, and night in winter, the general increase of temperature and length of day as the sun approaches the elevated pole, and the reversal of the seasons in the northern and southern hemispheres, are all facts too well known to require further comment."

The law of the distribution of heat, which, according to Sir Robert Ball's book (p. 118), had escaped the attention of Herschel, is here stated, not with mathematical accuracy, because it was thought too well known to need more elucidation, but with quite unquestionable clearness.

The paragraph from Herschel's *Outlines*, quoted and corrected in *The Cause of an Ice Age*, does not appear in the earlier editions; it seems to have been printed for the first time in the fifth. In the fourth edition, published in 1851, Article 368 was as follows:—

"(368.) The elliptic form of the earth's orbit has but a very trifling share in producing the variation of temperature corresponding to the difference of seasons. This assertion may at first sight seem incompatible with what we know of the laws of the communication of heat from a luminary placed at a variable distance. Heat, like light, being equally dispersed from the sun in all directions, and being spread over the surface of a sphere continually enlarging as we recede from the centre, must, of course, diminish in intensity according to the inverse proportion of the surface of the sphere over which it is spread; that is, in the inverse proportion of the square of the distance. But we have seen (art. 350) that this is also the proportion in which the *angular velocity* of the earth about the sun varies. Hence it appears, that the *momentary supply of heat* received by the earth from the sun varies in the exact proportion of the angular velocity, *i.e.* of the *momentary increase of longitude*: and from this it follows, that equal amounts of heat are received from the sun in passing over equal angles round it, in whatever part of the ellipse those angles may be situated. Let, then, S represent the sun; A Q M P the earth's orbit; A its nearest point to the sun, or, as it is called, the *perihelion* of its orbit; M the farthest, or the *aphelion*: and therefore A S M the *axis* of the ellipse. Now, suppose the orbit divided into two segments by a straight line P S Q, drawn through the sun, and anyhow situated as to



direction; then, if we suppose the earth to circulate in the direction P A Q M P, it will have passed over 180° of longitude in moving from P to Q, and as many in moving from Q to P. It appears, therefore, from what has been shown, that the supplies of heat received from the sun will be equal in the two segments, in whatever direction the line P S Q be drawn. They will, indeed, be described in unequal times; that in which the perihelion A lies in a shorter, and the

other in a longer, in proportion to their unequal area: but the greater proximity of the sun in the smaller segment compensates exactly for its more rapid description, and thus an equilibrium of heat is, as it were, maintained. Were it not for this, the excentricity of the orbit would materially influence the transition of seasons. The fluctuation of distance amounts to nearly $\frac{1}{30}$th of its mean quantity, and, consequently, the fluctuation in the sun's direct heating power to double this, or $\frac{1}{15}$th of the whole. Now, the perihelion of the orbit is situated nearly at the place of the northern winter solstice; so that, were it not for the compensation we have just described, the effect would be to exaggerate the difference of summer and winter in the southern hemisphere, and to moderate it in the northern; thus producing a more violent alternation of climate in the one hemisphere, and an approach to perpetual spring in the other. As it is, however, no such inequality subsists, but an equal and impartial distribution of heat and light is accorded to both."

In the fifth edition, published in 1858, this Article was replaced by the three following:—

"(368 *a.*) Let us next consider how these phænomena are modified by the ellipticity of the earth's orbit and the position of its longer axis with respect to the line of the solstices. This ellipticity (art 350) is about one sixtieth of the mean distance, so that the sun, at its greatest proximity is about one thirtieth of its mean distance nearer us than when most remote. Since light and heat are equally dispersed from the sun in all directions, and are spread, in diverging, over the surface of a sphere enlarging as they recede from the center, they must diminish in intensity according to the inverse proportion of the surfaces over which they are spread, *i.e.* in the inverse ratio of the squares of the distances. Hence the hemisphere opposed to the sun will receive in a given time, when nearest, two thirtieths or one fifteenth more heat and light than when most remote, as may be shown by an easy calculation. Now, the sun's longitude when at its least distance from the earth (at which time it is said to be in perigee and the earth in its perihelion) is at present 280° 28′ in which position it is on the 1st of January, or eleven days after the time of the winter solstice of the northern hemisphere; or, which is the same thing, the summer solstice of the southern (art. 364), while on the other hand the sun is most remote (in apogee or the earth in its aphelion), when in longitude 100° 28′ or on the 2nd of July, *i.e.* eleven days after the epoch of the northern summer or southern winter solstice. We shall suppose, however, for simplicity of explanation, the perigee and apogee to be coincident with the solstice. At and about the southern summer solstice then, the whole earth is receiving *per diem* the greatest amount of heat that it can receive, and of this the southern hemisphere receives the larger share, because its pole and the whole region within the antarctic circle is in perpetual sunshine, while the corresponding northern regions lie in shadow. On the other hand, at and about the northern summer solstice, although it is true that the reverse conditions as to the regions illuminated prevail, yet the whole earth is then receiving *per diem* less heat owing to the sun's remoteness: so that on the whole *if the seasons were of equal duration*, or in other words, if the angular movement of the earth in its elliptic orbit were uniform, the southern hemisphere would receive more heat *per annum* than the northern, and would consequently have a warmer mean temperature.

"(368 *b.*) Such, however, is not the case. The *angular* velocity of the earth in its orbit, as we have seen (art. 350), is not uniform, but varies in the inverse ratio of the square of the sun's distance, that is, in the same precise ratio as his heating power. The momentary supply of heat then received by the earth in every point of its orbit varies exactly as the momentary increase of its longitude, from which it obviously follows, that equal amounts of heat are received from the sun in passing over equal angles round it, in whatever part of the ellipse those angles may be situated. Supposing the orbit, then, to be divided into two segments by any straight line drawn through the sun, since equal angles in longitude (180°) are described on either side of this line, the amount of heat received will be equal. In passing then from either equinox to the other, the whole earth receives equal amount of heat, the inequality in the intensities of solar radiation in the two intervals being precisely compensated by the opposite inequality in the duration of the intervals themselves; which amounts to about $7\frac{1}{2}$ days, by which the northern spring and summer are together longer than the southern. For these intervals are to each other in the proportion of the two unequal segments of the whole ellipse into which the line of the equinoxes divides it (see art. 353).

"(368 *c.*) In what regards the comfort of a climate and the character of its vegetation, the intensity of a summer is more naturally estimated by the temperature of its hottest day, and that of a winter by its sharpest frosts, than by the mere durations of those seasons and their total amount of heat. Supposing the excentricity of the earth's orbit were very much greater than it actually is; the position of its perihelion remaining the same; it is evident that the characters of the seasons in the two hemispheres would be strongly contrasted. In the northern, we should have a short but very mild winter, with a long but very cool summer—*i.e.* an approach to perpetual spring; while the southern hemisphere would be inconvenienced and might be rendered uninhabitable by the fierce extremes caused by concentrating half the annual supply of heat into a summer of very short duration and spreading the other half over a long and dreary winter, sharpened to an intolerable intensity of frost when at its climax by the much greater remoteness of the sun."

Now, it is quite clear that Sir Robert Ball's suggestion attributing the origin of the mis-statement to Sir J. Herschel's having hastily confused two very distinct things, can by no means have been correct; because the remarkable proposition mentioned on page 120 of *The Cause of an Ice Age* was stated very clearly in the paragraph of the *Outlines* next preceding that which Sir R. Ball incriminates; and there Sir John Herschel was dealing with actual fact, whereas in the subsequent paragraph he was considering a supposititious case.

Surely it is more reasonable to suppose that he used the word *half* (with extraordinary lack of his usual carefulness), instead of *part*, to import mere division, without regard to quantity. This use of the word cannot possibly be defended, but the most careful writers are liable to similar slips; and it seems inconceivable that he could have meant to represent the heat received by either hemisphere during the year as equally divided between summer and winter, under any circumstances whatever. He includes heat and light as subject to the same law of diffusion, and who could possibly think or say that the summer and winter light could ever be equal?

We need not look far for inaccuracies somewhat analogous. The quotation from Sir Robert Ball, first above cited, reads as if he accused Herschel of formally teaching the alleged equality to be the normal or constant state of matters, but his indictment can hardly be meant to go so far as that. His book speaks of the mistake as an inadvertence (page 135).

Again, on page 27 of *The Cause of an Ice Age*, he says, "We have learned that one hemisphere was once covered

with ice," but this cannot mean, although it appears to say, that the ice-cap ever reached the equator in either hemisphere. Suppose that someone, reading it literally, were to maintain this to be the author's belief, would not Sir Robert Ball be entitled to call the assertion monstrous? Two pages before, he had spoken of "the sunny regions which seem never to have been invaded by the desolation of an Ice Age"; afterwards he twice mentions a huge sheet of ice over "a large part of" one or the other hemisphere; and, in the concluding chapter, speaks of the great ice-sheet as invading central Europe as far as Saxony, covering the greater part of Great Britain, submerging Canada, and burying much of that tract which now forms the Eastern States of North America. All these passages are just as incompatible with belief in an ice-cap extending to the equator, as the whole tenor of Sir John Herschel's teaching is with alleging the equality of summer and winter mean temperature.

There was once some difference between the teaching of Sir John Herschel and that of Sir Robert Ball about the astronomical theory of the Ice Age, but comparison of the articles above quoted from the fourth and fifth editions of Herschel's *Outlines* will show that the author deliberately withdrew his earlier contention that the eccentricity of the earth's orbit did not materially affect the transition of seasons. The statement in the later editions seems to anticipate Dr. Croll's and Sir Robert Ball's theory, that ice-ages can happen only when, the eccentricity being at or near its maximum, the line of equinoxes is perpendicular to the major axis of the earth's orbit, or nearly so.

Strangely enough, Dr. Croll, who is supposed to have submissively adopted Herschel's teaching, appears to have been unaware of this change, although it was made seventeen years before the publication of *Climate and Time*: for in his introduction to that book, and also in its first appendix, he quotes and criticises article 368, as it appeared in the earlier editions of the *Outlines*.

It is necessary to mention only two passages in *Climate and Time*, in order to prove that Dr. Croll was not one of those who have asserted the heat received from the sun during summer to be equal to that received during winter. On page 55 he says that change in the eccentricity of the earth's orbit may affect climate by increasing or diminishing the difference between summer and winter temperature; thus showing his knowledge of the familiar fact that some difference does, and always must, exist. And on page 87 he twice uses the relation of 7 to 4 in dealing with this difference; applying it in the first instance to the proportion between summer and winter sunshine in the latitude of Edinburgh, and in the second to the proportion between winter and summer nights in either hemisphere. Is not the ratio of 7 to 4 sufficiently near that of Sir Robert Ball's 63 to 37, for the purpose of protecting Dr. Croll from the imputation of having carelessly adopted the error, inadvertently suggested by a statement which there is some reason for believing that he had never seen?

Your obedient servant,
B. Noble.

Newcastle upon Tyne,
17th February, 1892.

CAMPHIRE AND CAMPHOR.

By J. Ch. Sawer, F.L.S.

In the 14th verse of the 1st chapter of the Song of Solomon we find: "My beloved is unto me as a cluster of Camphire in the vineyards of Engedi." At first sight the meaning of this verse is obscure. The word translated "Camphire" is certainly not intended to convey to the mind any idea of the substance we now know as Camphor; Solomon was very happy in his choice of similes, and such a comparison would have been absurd. In the original of this poem or love-song, written about 1000 years B.C., the word translated Camphire is *Copherim*, the Egyptian equivalent of which is *Hennah*. In Egypt, on one of the nights before a wedding, "Hennah" is applied with linen bandages to the hands and feet of the bride until the next morning, when they appear of that rosy red which Egyptians believe to be love's proper hue. This night, in the order of the marriage ceremonials, is called "the night of the Hennah." The word "cluster," found in the text, no doubt refers to the flowers of this plant, which are of a golden yellow and are borne in clusters; they are remarkably fragrant whether fresh or dry, and were much esteemed by women in the East, especially the Jewish women, who carried bouquets of them in their bosoms and twined them into crowns for their heads.

This Hennah is Pliny's "Cyprus of Egypt," and the women of Egypt and other Eastern countries stain not only the palms of their hands and the soles of their feet with a paste of Hennah leaves, but also the tips of the fingers, the nails and the knuckles, from which custom probably arises the designation of Aurora as "rosy," or "rosy-fingered" (ῥοδοδάκτυλος ἠώς).

A considerable business is done in the leaves of the Hennah, which are collected in the green state and dried in the sun. The leaves are then coarsely powdered and beaten up with Catechu, and the freshly made paste is laid on at bed-time and renewed in the morning; it leaves a peculiar reddish-orange stain, ruddy, and somewhat similar to the colour of red ochre, though hardly so deep, which lasts on the skin and nails for some three or four weeks, until removed by renewed growth. This colouration is much admired by Mussulmans in India and many Eastern countries.

On moistening the dried leaves a slight odour is perceptible; their taste is bitter and faintly aromatic, owing to an essential oil and tannin contained in them. A medicament prepared from them is employed by the Arabs in the treatment of wounds of all kinds, causing the skin to grow over and healing them very quickly; these effects are probably due to some exciting action of the essential oil and the astringent properties of the tannin. They use this remedy principally on horses, to heal wounds or sores caused by friction of the harness or otherwise on a journey. Having stopped for a rest and unsaddled, they will apply a plaister to it and continue their march without more thought of the wound, which, if it does not heal, at least does not extend by friction, and causes less suffering to the horse. They also employ it as a means of preventing the opening of old wounds where the hair has not grown over the scar. It closes and hardens the tissues—in fact, tans the skin. For similar reasons, the Arabs who can afford to indulge in the sport of gazelle hunting will give their horses a foot-bath of Hennah, especially if the animals are young, or have not taken exercise for some time. There may be some analogy to this in the custom prevailing amongst Arabian women of staining the palms of their hands and the soles of their feet—it may render the skin less tender.

This shrub, being known to the Arabs as "Henné-al-hennah," appears to be a native of Arabia. It has been cultivated from its earliest times, and is now very common throughout India, Cabul, and Persia, as well as along the coast of the Mediterranean. Botanically, it is now known to us as the *Lawsonia inermis* of Linnæus. It flowers and seeds most of the year, and is much used for hedges,

growing readily from cuttings. It has been introduced into the West Indies, and is there known as Jamaica Mignonette. The perfume of the flowers is rather that of a mixture of rose and mignonette. Acids destroy the dye yielded by the leaves, but alkalies and infusions of astringent plants deepen it; although this juice stains the epidermis, it does not communicate any colour to cloth.

The name of the white crystalline substance commonly known as Camphor is derived from the Arabic word "Káfûr," which in its turn was derived from the Sanskrit word "Kapûra," signifying *white*, or a pure substance.

The old English name for Camphor was spelt *Camphire*, and as the translators of the Bible may have been unacquainted with the botany and natural history of the plant Hennah, they may have confounded the Hebrew word "Cophérim," or "Kopher," with the Arabic word "Káfûr." This is pure surmise, but evidently the word "Hennah" should be substituted for the word "Camphire" in the Song of Solomon. Moreover, there is no record of the substance Káfûr, or Kápûra, being known in Solomon's day.

Camphor is first mentioned by Arabian writers in the sixth century. It is mentioned by Aëtios, of Amida, in Mesopotamia, according to whom Kaphura is a rare and wonderful medicine. It is again mentioned, together with musk, amber, and santal wood, among the treasures taken in the year 636, by the Kalif Omar, at the plundering of the Sassanides Palace, in Madain, on the Tigris, and is subsequently noticed as a costly gift, often presented by Indian princes to high Chinese officials. This Camphor came from the land known as Kaïsûr, the present Sumatra.

Ishak Ibn Amrân, an Arabian physician, who lived towards the close of the ninth century, states that the best Camphor was produced in Fansûr, a locality which was visited by Marco Polo in the thirteenth century, who mentions that the Kaïsûr Camphor was then marketable at its weight in gold.

From various sources of information it may be concluded that Camphor, as it was first known, was the variety which exists ready formed in the pith cavities of the trunk of the *Dryobanalops camphora*, a magnificent tree growing in Borneo, Sumatra, and Labua. In the forests of Sumatra these trees attain an immense size, often being found of 6 or 7 feet in diameter. They do not all contain Camphor, many of them containing an oil, which is supposed to be the first stage of the formation of the drug, and would develop into Camphor were the tree left unmolested. Both oil and Camphor are found in the heart of the tree, not occupying the whole length of the pith cavity, but often in spaces of a foot or a foot and a half in length, at intervals. The method of extracting the oil is merely by making a deep incision with a Malay axe, about 14 or 18 feet from the ground, till near the heart, when a narrower incision is made, and the oil, if any in the tree, gushes out, and is received in bamboos or other utensils. In this manner a party proceeds through the woods, wounding the Camphor trees till they attain their object. From a tree containing both oil and Camphor, 2 gallons of the former and 3 lbs. of the latter may sometimes be obtained, but hundreds of trees may be mutilated before Camphor is discovered, as the natives have no certain means of ascertaining if the tree produces either the one or the other. When Camphor is found, the tree is felled and cut into junks of a few feet long; these are then split, and the Camphor is found in the heart, occupying a space in circumference of the thickness of a man's arm. The quantity varies from 3 to 15 lbs., and rarely as much as 20 lbs. are obtained. Some trees when felled are not found to contain any at all. The Camphor thus found is called *Tentary*, and by reason of the small quantity, it commands a high price—from 35 to 70 shillings a pound, according to quality. It does not find a market in Europe at all, but is used to some extent on the spot as incense, especially in the observation of funeral rites and embalming the bodies of the dead, and is exported to China, Japan, and other places in Eastern Asia, for similar purposes. It is heavier than Laurel Camphor, and sinks in water.

The date at which the Chinese discovered the production of Camphor from the *Laurus camphora* is unknown. This is called Laurel Camphor, or "Common" Camphor. It was brought into Europe by the Arabians about the twelfth century, which is proved by the mention made of it by the Abbess Hildegard ("St. Hildegardis Opera omnia," 1145, published in Paris 1855) who called it Ganphora. Garcia de Orta, writing in 1563, states that the Chinese is the only Camphor imported into Europe; that of Borneo and Sumatra, being a hundred times more valuable, is retained by the Orientals for their own use. Kämpfer (strange coincidence of names), who visited Japan 1690–92 and made a drawing of the Japanese Camphor tree under the name of *Laurus camphòrifera*, expressly declares that the tree differs entirely from the Camphor-yielding tree of the Malay Archipelago. He further states that the Borneo Camphor is one of the most precious articles of merchandise imported into Holland from Japan. This Camphor was refined in Japan by a process long kept secret.

The common Camphor tree, *Laurus camphora*, is distributed throughout the eastern provinces of Central China, on the Island of Hainan, and very extensively in Formosa. It also occurs as a forest-tree on the islands Kiushiu and Shikoku of South Japan, its growth being much more vigorous there than in the more northern districts. The Camphor of European commerce is produced almost exclusively from the Camphor laurels of Formosa and Japan.

The large and increasing quantities of this drug consumed in all civilized countries make the question of its continued production and regular supply a matter of considerable importance. It is a well-known fact that the distillation of the crude Camphor from the wood is conducted in a primitive careless way, which causes great waste. The Camphor laurels of Formosa are gradually being destroyed under the careless system employed by the Chinese gatherers. In fact they have been entirely exterminated along the sea-board, and the wood is now obtained in the forests along the frontier, between the settlements of the Chinese and the inland mountainous regions still occupied by the aboriginal population. The Camphor-gatherers are hence continually exposed to the assaults of the natives, which interrupt the profitable prosecution of this industry. No attempts are made to cultivate laurels to take the place of those destroyed, and a sufficient quantity of the drug is only obtained by constant encroachments upon the territory of the Formosans, destroying the trees still further into the interior at every new move.

The method of extracting the Camphor is as follows:— The trees are felled and the small branches chopped up; these, with the chips and twigs, are alone used, the heavy wood being abandoned. A long trough, made of a hollow tree, and coated with clay, is placed over eight or ten hearth fires, and is half filled with water. Boards, perforated with holes, are put across the trough, and above each hole is a jar filled with chips of the wood, with

earthenware pots inverted above them, the joints being made tight by hemp and clay. The water in the trough is heated to boiling, and the steam passing through the holes saturates the chips, causing the Camphor to sublime and condense in crystals in the inverted pots above. The Camphor thus obtained is sent into the interior of the island, to Tasmin, the principal port, packed in baskets covered with cloths and large leaves. On arrival, it is re-packed in tubs or lead-lined cases for export by Chinese vessels to Hong Kong, Shanghai or Canton, the loss by evaporation while in transit from the place of its production being very large. A yellow oil exudes from the packages of this crude Camphor, locally known as "Oil of Camphor," and is used medicinally. The Formosa Camphor, which sometimes goes by the name of "Chinese Camphor," occasionally arrives in India in a semi-fluid state, owing to the addition of water before shipment.

The Japan Camphor used to be extracted, according to Kämpfer (the authority above referred to), by boiling the wood with water in an iron kettle, and condensing the vapour in an earthenware dome, closed at the top with rice-straw. The modern practice is to distil the wood with water in an iron retort fitted with a wooden dome, from which the vapours are led through a bamboo tube to the cooling apparatus. This consists of a wooden box, containing seven transverse compartments, and is enclosed in a second box through which water is allowed to flow; the vapours are conducted through all the compartments in succession by means of holes placed alternately at either end of the dividing walls. The Japan Camphor arrives dry; it is lighter in colour, and somewhat pinkish. It arrives in double tubs (one within the other) without metal lining; hence it is sometimes called "tub-Camphor."

The European process of refining Camphor was long kept a secret, and towards the end of the seventeenth century the entire Camphor of Europe had to be sent to Holland to be sublimed. A monopoly was also held for some time in Venice, but at the present day Camphor refining is largely accomplished in England, Holland, Hamburg, Paris, New York and Philadelphia. Before describing the correct European method, it may be well to describe the fraudulent method adopted in India, the artful peculiarity of which is to get as much interstitial water as possible into the Camphor cake. The vessel used is a tinned cylindrical copper drum, one end of which is removable; into this is put 14 parts of crude Camphor, and $2\frac{1}{2}$ parts of water; the cover is then luted with clay, and the drum being placed upon a small furnace made of clay is also luted to the top of the furnace. In Bombay, four of such furnaces are built together, so that the tops form a square platform. The sublimation is completed in about three hours; during the process, the drums are constantly irrigated with cold water. Camphor sublimed in this way is not stored, but distributed at once to the store-keepers before it has had time to lose weight by drying. It is sold at the same price as the crude article, the refiner's profit being derived from the introduction of water. The same practice seems to be followed at Delhi, and at a few other cities in India.

In Europe, it is usually refined by mixing it with lime, charcoal, or iron filings, and subliming the mixture in large glass vessels; cakes weighing eight to twelve pounds being thus obtained.

The process adopted in Philadelphia is devised in such a way as to obtain the sublimate in the form of a finely powdered snowy mass, to accomplish which, about one-tenth per cent. of water is added to the crude material. The apparatus consists of a flat iron chamber, capable of holding about 200 lbs., connected by means of an iron tube with a condensing chamber eight feet long, four feet wide, and four feet high. This chamber is constructed of enamelled bricks set in Portland cement, forming an arched roof and floor of the same material. After an operation the apparatus is allowed to remain undisturbed over night, to become sufficiently cool. On removal of the sublimate, it is compressed into moulds by hydraulic pressure of 2500 lbs. to the square inch, and the finished product obtained in small cakes, highly compressed, and weighing one ounce.

Camphor forms a tough crystalline mass of characteristic taste and odour, and can only be powdered when it is moistened with alcohol or some other solvent. It dissolves in 1300 parts of water at 20° C., and at 12° C. in 0·8 parts of alcohol of sp. gr. 0·806. It is readily soluble in ether, acetone, chloroform, benzine, and other hydro-carbons; also in glacial acetic acid and in carbon disulphide. It melts at 175° C., and boils at 204° C., but volatizes very rapidly at the ordinary temperature and sublimes, when kept in close vessels, in lustrous hexagonal crystals which frequently form splendid stars.

Camphor oil is used for mixing with fine lac varnishes, rendering them less liable to crack. It is a powerful antiseptic and disinfectant, and covers the smell of mineral oils.

THE FACE OF THE SKY FOR MARCH.

By Herbert Sadler, F.R.A.S.

Sun-spots are rapidly increasing in number and size, a group being distinctly visible to the naked eye at the time of writing these lines. The following are conveniently observable minima of some Algol-type variables (*cf.* "Face of the Sky," for February). Algol.—March 10th, nine minutes after midnight; March 13th, 8h. 58m. P.M. S Cancri.—March 16th, 11h. 46m. P.M. The student will, of course, keep a watch on Nova Aurigæ, at present about the fifth magnitude, the place of which for 1892 is 5h. 25m. 3s. +30° 21′. Its spectrum seems to resemble in some particulars those shown by Novæ 1866 and 1876.

Mercury is in superior conjunction with the Sun on the 6th, and after that he rapidly improves in position, setting on the 16th at 7h. 1m. P.M., 56m. after the Sun, with an apparent diameter of $5\frac{1}{2}''$, and a northern declination of 2° 15′, $\frac{92}{100}$ths of the disc being illuminated. On the 21st he sets at 7h. 38m. P.M., or 1h. 24m. after the Sun, with an apparent diameter of 6″, and a northern declination of 6° 53′, $\frac{80}{100}$ths of the disc being illuminated. On the 26th he sets at 8h. 9m. P.M., or 1h. 47m. after sunset, with an apparent diameter of $6\frac{1}{2}''$, and a northern declination of 10° 56′, $\frac{63}{100}$ths of the disc being illuminated. On the 31st, when he is at his greatest eastern elongation (18° 54′), he sets at 8h. 28m. P.M., or just upon two hours after the Sun, with an apparent diameter of $7\frac{1}{2}''$, and a northern declination of 13° 58′. The student may be reminded that Mercury is at his greatest brilliancy about ten days or a fortnight before his greatest eastern elongation, notwithstanding that at his greatest eastern elongation he sets considerably later in the evening. While visible, Mercury describes a direct path through Pisces to the borders of Aries.

Venus is now becoming a very bright object in the evening sky. She sets on the 1st at 9h. 18m. P.M., with a northern declination of 7° 52′, and an apparent diameter of $14\frac{1}{2}''$, $\frac{76}{100}$ths of the disc being illuminated. On the 21st she sets at 10h. 18m. P.M., with a northern declination of 17° 8′, and an apparent diameter of $16\frac{1}{2}''$, $\frac{69}{100}$ths of the

disc being illuminated, and her brightness being about one-half of what it will be at the beginning of June. On the 31st she sets at 10h. 48m. P.M., with a northern declination of 20° 53′, and an apparent diameter of $17\frac{1}{2}''$, $\frac{65}{100}$ths of the disc being illuminated. In the early evening of the 28th an $8\frac{1}{2}$ magnitude star will be situated very near the planet. During the month she passes from Pisces, through Aries, into Taurus. Mars is invisible, and Jupiter is in conjunction with the Sun on the 21st.

Saturn is well-placed for observation, being in opposition to the Sun on the 16th, at a distance from the earth of about $787\frac{1}{4}$ million miles. He rises on the 1st at 6h. 55m. P.M., with a northern declination of 3° 5′, and an apparent equatorial diameter of 19·2″ (the major axis of the ring system being 44·1″ in diameter, and the minor 1·9″). On the 31st he rises at 4h. 45m. P.M., with a northern declination of 4° 2′, and an apparent equatorial diameter of 19·2″ (the major axis of the ring system being 44·1″ in diameter, and the minor 1·1″). The following phenomena of the satellites may be observed (the times are given to the nearest quarter of an hour). March 3rd, $8\frac{1}{4}$h. P.M., Rhea, eclipse disappearance. March 4th, $3\frac{3}{4}$h. A.M., Titan, eclipse disappearance. March 5th, $0\frac{3}{4}$h. A.M., Dione, eclipse disappearance. March 6th, $4\frac{1}{4}$h. A.M., Tethys, eclipse disappearance. March 9th, $1\frac{1}{2}$h. A.M., Tethys, eclipse disappearance; 6 P.M., Iapetus, at greatest W. elongation. March 10th, 11h. P.M., Tethys, eclipse disappearance. March 11th, $10\frac{1}{2}$h. P.M., shadow of Titan in central transit. March 12th, $8\frac{1}{4}$h. P.M., Tethys, eclipse disappearance; 9h. P.M., Rhea, eclipse disappearance. March 18th, $8\frac{1}{2}$h. P.M., Dione, eclipse reappearance. March 22nd, $1\frac{1}{2}$h. A.M., Rhea, eclipse reappearance. March 26th, $4\frac{1}{4}$h. A.M., Tethys, eclipse reappearance. March 27th, $1\frac{1}{2}$h. A.M., Dione, eclipse reappearance; $9\frac{3}{4}$h. P.M., shadow of Titan in central transit. March 28th, $1\frac{1}{2}$h. A.M., Tethys, eclipse reappearance. March 29th, 7h. P.M., Dione, eclipse reappearance; $10\frac{3}{4}$h. P.M., Tethys, eclipse reappearance. March 31st, $2\frac{1}{2}$h. A.M., Rhea, eclipse reappearance; $8\frac{1}{4}$h. P.M., Tethys, eclipse reappearance. At 10h. P.M. on the 17th, a 9th magnitude star will be about $1\frac{1}{2}'$ north of the planet. During March Saturn describes a short retrograde path in Virgo, without approaching any naked-eye star.

Uranus is coming into a good position for observation, rising on the 1st at 10h. 37m. P.M., with a southern declination of 13° 0′, and an apparent diameter of 3·7″. On the 31st he rises at 8h. 34m. P.M., with a southern declination of 12° 43′. He will be so favourably placed for observation during March, never being more than 30′ from the $4\frac{1}{2}$ magnitude star λ Virginis, that the student should endeavour to pick the planet up with the naked eye or an opera glass. His occultation by the Moon is mentioned below. A map of the path of Uranus is given in the *English Mechanic* for February 12th. Neptune is still visible, rising on the 1st at 9h. 43m. A.M., with a northern declination of 19° 50′, and an apparent diameter of $2\frac{1}{2}''$. On the 31st he sets at 11h. 36m. P.M., with a northern declination of 19° 56′. He describes a short direct path to the N.W. of ε Tauri.

There are no very well-marked showers of shooting stars in March. The zodiacal light should be looked for over the western horizon on every moonless evening.

The Moon enters her first quarter at 7h. 15m. P.M. on the 5th; is full at 0h. 55m. P.M. on the 13th; enters her last quarter at 5h. 16m. P.M. on the 21st; and is new at 1h. 18m. P.M. on the 28th. She is in apogee at 9·7h. P.M. on the 15th (distance from the earth 252,300 miles); and in perigee at 10h. P.M. on the 28th (distance from the earth 221,960 miles). The greatest western libration is at quan.10m. A.M. on the 7th, and the greatest eastern at 3h. 14m. A.M. on the 23rd. The planet Uranus, equal to a $5\frac{1}{2}$ magnitude star, will be occulted at 0h. 30m. A.M. on the 17th, at an angle of 112°, reckoned as in double star observation, *i.e.*, from the true N. point in the direction N.E.S.W., and will reappear at 1h. 46m. A.M., at an angle of 318°.

Chess Column.

By C. D. Locock, B.A.Oxon.

All communications for this column should be addressed to the "Chess Editor, *Knowledge Office*," and posted before the 10th of each month.

The solution of the February problem is withheld for the same reason as in the case of the previous problem.

Correct Solutions have been received from Giuoco Pianissimo, W. T. Hurley, and A. Rutherford. Of the MS. Problem, solutions in three moves from W. T. Hurley, A. Rutherford, and M. B. (Jesmond): the latter not quite complete.

R. G. Haig (*Melbourne*).—In the variation you mention the Queen mates at QRsq. This is probably the leading variation, and no doubt you will have seen the mate before this.

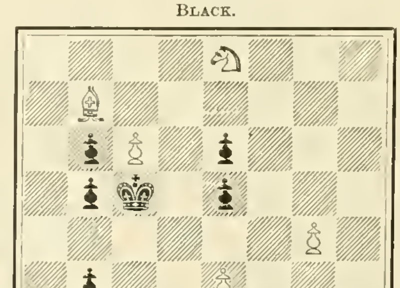
Alpha.—The move you rejected is, strange to say, the correct key-move, though rather strong-looking. In reply to the move you suggest, Black must not play 1. . . P × Kt, or 1. . . . P to B3; but he may play apparently almost anything else. Certainly there is not much pleasure in finding more than one solution to any problem.

M. B. (*Jesmond*).—In the three-move problem you omitted the important defences, Q to Kt6 (compelling Q to Kt4ch) and P to K5. Also the defence R to Kt3, in which a slight variation in the mating method occurs. After 1. . . . K × P, Kt to B5 is a *triple* continuation. In position A there is a dual on the third move—after 1. . . P becomes a Kt, etc.

PROBLEM.

[From *England*.—Composer unknown.]

Black.



White.

White to play, and mate in three moves.

"KNOWLEDGE" SOLUTION TOURNEY.

This contest has at length been decided. It will be seen from the list of correct solutions that, of the four surviving competitors, two only proved equal to the double task set last month. These two were Mr. A.

Rutherford, of Liverpool, and Mr. W. T. Hurley, of Rochester. Giuoco Pianissimo, besides solving the Four-move problem, found two solutions in four moves to the MS. problem. But as the problem was solvable in three moves, these solutions could not count. He was thus, strange to say, defeated by his own suggestion, that the number of moves should not be stated. This paradoxical result occurred also in the case of Mr. Hurley. The scoring of duals was his own suggestion and the sole cause of his defeat, as the following score will show:—

Solutions—A. Rutherford, 16; W. T. Hurley, 16.

Duals—A. Rutherford, 12; W. T. Hurley, 5.

Other Variations—A. Rutherford, 3; W. T. Hurley, 4.

Mr. Rutherford, of the Liverpool Chess Club, thus comes out the winner of the Knowledge Prize, and considering the quality of the opposition, must be congratulated on a really fine performance. Messrs. Hurley, Giu. Pianissimo and M. B. (Jesmond), also deserve congratulation, not unmingled with condolence, on the excellent fight they made. This award remains open for one week.

CHESS INTELLIGENCE.

The championship match at Havana will, perhaps, be over before the appearance of this number. At the time of writing the score is—Steinitz 7, Tschigorin 8, Drawn 4. Mr. Steinitz has been singularly unsuccessful against the Evans Gambit and the Two Knights Defence. Most of his victories have been obtained, as usual, in the Ruy Lopez and the Close Game. It is well known that Mr. Steinitz always reserves his finest play for the latter stages of an important contest, and it will therefore cause some surprise if he loses the match.

The contest for the championship of the City of London Club resulted in a tie between Messrs. Moriau, Mocatta and Dr. Smith, and is now reduced to a duel between the first and last named.

The British Chess Club Handicap is still undecided, pending the decision of a committee appointed to adjudicate on several unplayed games.

Game played by telephone on December 12th, 1891:—

[Zukertort Opening.]

White (Liverpool). (Rev. J. Owen, Messrs. Cairns, Howard, and Kaizer.)	Black (British Chess Club). (Messrs. Donisthorpe, Hoffer, G. Newnes, M.P., and Trenchard.)
1. Kt to KB3	1. P to Q4
2. P to Q4	2. P to K3
3. P to K3	3. P to QB4 (*a*)
4. P to QKt3	4. Kt to KB3
5. B to QKt2	5. Kt to QB3
6. B to Q3	6. P to QKt3 (*b*)
7. Castles	7. B to Q3 (*c*)
8. QKt to Q2	8. Castles
9. P to QR3 (*d*)	9. B to Kt2
10. Q to K2	10. R to Bsq (*e*)
11. Kt to K5	11. Q to B2
12. P to KB4	12. P × P (*f*)
13. P × P	13. Kt to K2
14. QR to QBsq (*g*)	14. Kt to Kt3
15. P to KKt4	15. KR to Ksq (*h*)
16. P to KKt5	16. Kt to Q2
17. Q to KR5	17. Kt (Q2) to Bsq
18. R to KB3	18. B × Kt (*i*)
19. KBP × B	19. R to K2
20. QR to KBsq	20. P to QKt4 (*j*)
21. QR to KB2	21. P to R4
22. Kt to KBsq	22. B to B3
23. Kt to K3	23. B to Ksq
24. Kt to Kt4	24. P to KB4 (*k*)
25. KtP × P (*en passant*)	25. R to B2
26. R to R3	26. P × P (*l*)
27. Kt × P(ch) (*m*)	27. K to Rsq
28. Q to KR6 (*n*)	28. R to Kt2
29. R to Kt2	29. P to QKt5
30. P to QR4 (*o*)	30. Q to K2
31. B to QBsq	31. Q to R2 (*p*)
32. B to K3	32. B to B2
33. K to Rsq	33. B to Ksq
34. Kt × B	34. R × Kt
35. B × Kt	35. Resigns (*q*).

Notes.

(*a*) Not advisable so early for the second player, if indeed for either side. The proper line of development is by P to QKt3 and B to Kt2.

(*b*) P to QR3 followed by P to QKt4 would be useless, as White could stop any further advances by P to QB4.

(*c*) B to Kt2 seems preferable. The Bishop is bound to occupy that square sooner or later, while the proper square for the King's Bishop cannot yet be determined.

(*d*) To prevent Kt to QKt5, since White intend on their next move to block up their Bishop's retreat. They might, however, play Kt to K5 at once.

(*e*) They should have prevented Kt to K5 by Q to B2.

(*f*) Making matters worse by relieving White of their weak King's Pawn, and at the same time allowing the White Queen's Bishop to come into play ultimately on the King's side. The open QB file is inadequate compensation, Perhaps Kt to K2, with a view to Kt to K5, was better.

(*g*) To prevent Kt to K5; for after the exchange of pieces Black would otherwise win the QBP. White now obtain an irresistible attack, the opposing forces being completely shut in.

(*h*) To make room for one of the Knights at Bsq in view of the coming onslaught.

(*i*) Black's best chance lay in 18 . . . Q to K2; if then 19. P to QR4, P to KB4; 20. P × P *en passant*, Q × P, with a fairly defensible game.

(*j*) In order to be able to reply to B to QR3 subsequently by P to Kt5.

(*k*) The only way to prevent the mate threatened by Kt to B6ch, followed, if the Knight be taken, by P × Kt and Q to R6; or, if the King move, by Kt × RP and R to R3.

(*l*) White threatened to win at once by B × Kt.

(*m*) Kt to R6ch was of course good enough, but this is even stronger.

(*n*) We fail to see the objection to 28. B × Kt, Kt × B? 29. Q × Kt. If Black then pins the Queen by R to Kt2, mate follows in two moves. Again, if 28 . . . R to Kt2!; 29. Kt × B, R × Kt; 30. R × Ktch, R × R; 31. R to Kt3, QR to KKtsq; 32. B to Q3, R × Rch; 33. P × R, Q to KKt2; 34. K to Rsq!, followed soon by B to QBsq, and B to KKt5. In this variation, if Black play 33. . . . R × Pch, White must reply 34. K to B2; not 34. K to R2?, R × B! and draws.

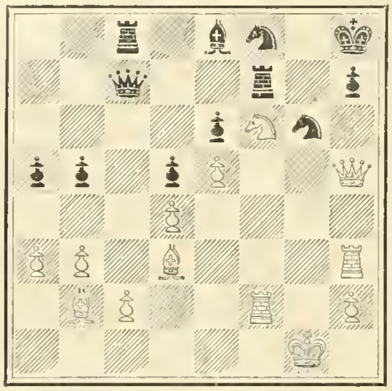
(*o*) If 30. Kt × B, R × Kt; 31. B × Kt, Kt × B; 32. R × Kt, R × R; Q × R, R to Ktsq, and wins.

(*p*) If the Queen stays where she is, the Bishop goes to Kt5.

(*q*) Apart, perhaps, from their 28th move, the Liverpool players conducted their attack in the finest possible style. Black, perhaps, missed their chance at the 18th move, but afterwards could not have done more than they did. They could only wait to be crushed.

Position after Black's 27th move.

BLACK.



WHITE.

Chess Endings. By E. FREEBOROUGH. (Messrs. Kegan Paul & Co.)—This work is a valuable addition to chess literature. Previously to its publication, the only modern treatise on end-games in the English language was that of Horwitz, comprising a collection of positions by Kling and Horwitz. These, however beautiful as problems or semi-problems, were for the most part hardly genuine end-games, the positions being unlikely to occur in actual play. The student was therefore driven to rely on the work of Herr Berger, the great German authority on endings; but at the same time he must have felt the want of something written in his native tongue, and (shall we say?) a little shorter. This want has been supplied by Mr. Freeborough, who, while of course largely indebted to the above-mentioned treatises, has succeeded in avoiding the defects of both. The book contains about 550 diagrams, of which the respectable proportion of 140 are devoted to King and Pawn Endings—by far the most important in our opinion. These are rightly placed first, in their natural position, not relegated to the end, as in Berger's work. The book, as far as we have examined it, is singularly free from misprints, the bane of Horwitz's work. We have noticed, however, two or three, and give them below, together with some suggested amplifications :—

No. 10.—Note 1. "If 2. . . . P to Kt4ch ; 3. K to Kt3." Add here, "And if then 3. . . . P to R4 ; 4. . . . K to Kt2, as in No. 14." In the same note, after the words "If otherwise to Kt3," a variation might be given : *e.g.*, 2. . . . P to Kt4 ; 3. K to Kt3, P to B5 ; 4. K to Kt4, P to R4ch ; 5. K to B3, and wins.

No. 15.—Here there is an error of some kind. The note implies that Black (with the move) wins by P to B4ch ; White however wins by K to R3.

No. 26.—"White wins through being able to advance his Pawn one or two squares" : *i.e.*, *even without the move.* With the move he wins apart from this privilege by P to Kt3.

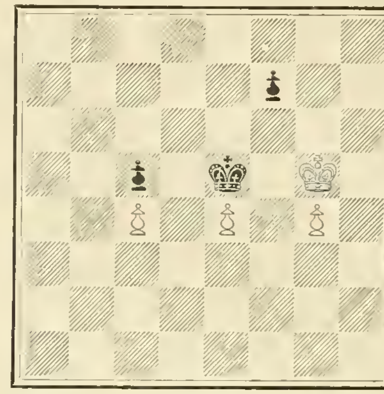
No. 31.—Black should have played 3. . . . P to Kt5, and drawn.

No. 34.—Add perhaps, If 1. . . . K to Q2 ; 2. P to B6.

No. 40.—Should be carried out further, the position, as Mr. Freeborough leaves it, being very instructive, and the win for Black not too obvious.

Position after Black's third move.

BLACK.



WHITE.

White now plays 4. K to R6. Black now wins as follows : 4. . . . K × P ; (K to Q5 only draws. For K to B5 see A.) 5. P to Kt5 (if 5. K to Kt7, P to B4 !). 5. . . . K to B5 ! ; 6. K to R4, K to B4 ; 7. K to R5 !, P to B3 ! ; 8. P × P, K × P and wins.

(A.) The following attempt to win would result in loss. After 4. K to R6, K to B5 ? ; 5. P to Kt5, K to Kt5 ; 6. P to K5 !, K to B4 ; 7. K to Kt7, K to K3 ; 8. K to B8, and wins.

No. 43, last note. Here, too, the analysis might well be continued : *e.g.*, "For if 3. . . . P to Kt5 ; 4. K to Q5 ; and if then both sides queen a Pawn, White wins the Queen in three moves. Again, if 3. . . . K to K3 ; 4. K to K4, K to B2 ; 5. K to Q5, &c.

No. 130.—Here, too, an error has crept in to the last note. 3. . . . P to Kt3 loses for Black by 4. P to KR4, P to Kt4 ; 5. P × P, K to Bsq ; 6. P to Kt6 and wins.

No. 148.—In this position a mate in three moves appears to have been overlooked.

No. 394.—The diagram is incorrect. White can mate in *one* move.

It is hoped that the above notes may be of some slight value to readers of the work, or even to the editor of it himself in the not unlikely event of future editions. It need only be said, in conclusion, that the arrangement of the whole book is lucid, the diagrams abundant and well-placed, and the printing both accurate and excellent.

CONTENTS OF No. 76.

TERMS OF SUBSCRIPTION.

"KNOWLEDGE" as a Monthly Magazine cannot be registered as a Newspaper for transmission abroad. The Terms of Subscription per annum are therefore as follows :—To any address in the United Kingdom, the Continent, Canada, United States, Egypt, India, and other places in the Postal Union, the Subscription is 6 shillings, including postage ; or 1 dollar 50 cents ; or 6, marks ; or 7 francs 50 cents.

For all places outside the Postal Union, 5 shillings in addition to the postage.

Communications for the Editor and Books for Review should be addressed Editor, "KNOWLEDGE" Office, 326, High Holborn, W.C.

KNOWLEDGE
AN ILLUSTRATED
MAGAZINE OF SCIENCE
SIMPLY WORDED—EXACTLY DESCRIBED

LONDON: APRIL 1, 1892.

CONTENTS.

BRITISH MOSSES.

By the Rt. Hon. Lord Justice Fry, F.R.S., F.S.A., F.L.S.

(*Continued from page 43.*)

But, it will be said, assuming that this may be the case with one growth of forest, how about the successive destruction of successive forests? The answer is, I believe, to be found in the curious change which peat undergoes, and which converts it from a substance highly absorbent of water into one impervious to it.

The section exposed by a peat-cutting in, I believe, almost all cases exhibits two kinds of peat, the one known variously as red peat—or red bog, or fibrous bog, or in Somersetshire as white turf—which lies at the top, and the other a black peat, which lies at the bottom. The red peat retains visible traces of the Sphagnum of which it is mainly composed, and is highly absorbent of moisture; whilst the black peat has lost all, or nearly all, traces of the minute structure of the cells, and is not only unabsorbent of moisture, but is impervious to it. In fact, it constitutes an insoluble substance which is said to be scarcely subject to decay, so that it is used in Holland for the foundations of houses, and is found unchanged after ages, and when the buildings have fallen into decay. It is even said to have remained unchanged after three months' boiling in a steam-engine boiler. The broad difference between these two kinds of peat may easily be ascertained by anyone who will, as I have done, subject the two kinds to the action of water.

In Fig. 33 will be found a section of a peat bog, copied from an engraving in the third Report of the Commissioners on Irish Bogs ("Parliamentary Papers," 1813-4), and exhibiting the remains of three forests anterior to the vegetation growing on the surface of the bog. The history of the formation will be, I believe, much as follows:—

(1) We must get a watertight bottom. In the section given it is said to consist of limestone gravel, but this probably had, at least in its lower part, got consolidated into a pan by the infiltration of insoluble iron oxides, themselves often due to decaying vegetable matter, or it rested on a subsoil of stiff clay. The necessity of this watertight bottom is well shown by the fact that in places in the Irish bogs where a pure limestone subsoil occurs the bog becomes shallow and dry.

(2) On this limestone gravel a forest arose and flourished for a considerable period, until the natural drainage of the area was stopped, whether by the choking up of the course of the effluent stream, or from the aggregation of vegetable matter, or from the fall in the course of nature of the trunks of the trees themselves. Everyone who will consider how much care our rivers require in order to make them flow with regularity to the sea—who thinks, for instance, of the works in the Thames valley, or in the upper valleys of the Rhine—will see how often and how easily, in a country in the condition of nature, stagnant waters will arise. In the morass thus formed the Sphagnum has grown, years after years, and if it has not destroyed the old trees it has prevented the growth of young ones. The stools of the trees buried in the antiseptic waters of the Sphagnum pools have been preserved, whilst the fallen trunks have, except when preserved by the like circumstance, rotted, and added their remains to the peat which

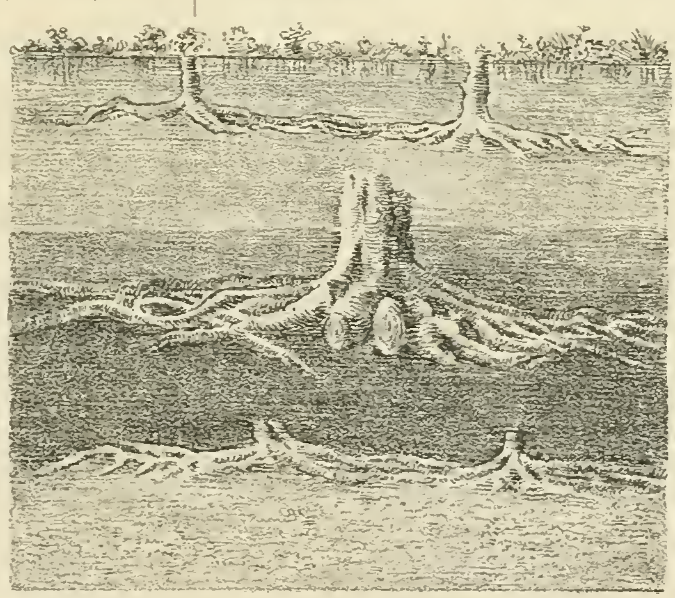


Fig. 33.—Section of Irish Peat bog, showing the growths of three successive submerged forests. (*From Third Report of Commissioners on Irish Peat Bogs.*)

the Sphagnum has been producing. It has been observed in several places in Scotland, that the under side of fallen trees which would be protected from decay by the tannin of the Sphagnum is preserved, whilst the upper side has decayed or rotted away. Year by year the process of decay on the lower parts of the Sphagnum went on until the water grew shallower and at last disappeared, leaving the original morass choked and filled up by the Sphagnum and the plants which it has nourished. On the top of this soil have grown first the heath and bog shrubs which first succeed the Sphagnum, and in time, as the soil has grown more solid, forest trees. This is our second forest. This first peat deposit, or the lower part of it at all events, having been turned into the black peat impervious to water, plays the same part in the second stage that the clay or pan did in the first stage. Again, the drainage of this second level got stopped, and the forest bottom loaded with stagnant water, the home of the Sphagnum; together, the water and the Sphagnum killed the forest trees, which share the fate of their predecessors. The same history is gone through again—the Sphagnum filling up the morass and turning the water into dry land until it supports the third forest.

Decay of the Moss.—There comes, however, in many cases a time when this process is arrested; the artificial drainage of the soil, or the physical position of the area, prevents the re-formation of a morass, and the Sphagnum dies away. So in many parts, if not universally, in Sedgmoor, in Somerset, it is almost impossible to gather a bit of Sphagnum, and the peat is well known to the turf diggers not to be reproduced. Here the regulated drainage of the level maintains the surface in the condition of meadows or agricultural land. But in many cases, especially on mountain sides or tops, when the Sphagnum has died, and the peat undergone its last change into black earth, a process of decay sets in under the influence of air and water. The water lies in holes or "hags," or flows in sluggish streams, wearing away the dead peat; and the surface of the soil is broken and uneven, small patches of green surface with a rough growth of sedge or grass being surrounded by wider spaces of black earth. Such is, or was some years ago, the condition of the peat on the top of Kinder Scout in Derbyshire; on the parts of Dartmoor around Cranmere Port; and such also it is described to be on many of the Lowland hills of Scotland.

Sedgmoor.—In some cases the Peat Mosses have been originally arms of the sea, and the peat has only grown after the exclusion of the salt water. Such appears to be the history of Sedgmoor, the great plain of Central Somerset. Northward it is bounded by the Mendips; eastward lies Glastonbury with its Tor or hill; westward the Bristol Channel. The plain is intersected by the low line of the Pouldon Hills, once a long level-backed island or promontory in the estuary and afterwards in the morass; and the way in which the villages lie and the moor is apportioned between them suggests that the Pouldon Hills and some other spots which slightly rise above the level of the Moss were the original seats of population. Originally this whole area appears to have been open to the Bristol Channel, of which it formed a bay or recess. The Burtle beds are a marine deposit well seen at the slight elevation on which the village of Burtle stands, which have been traced in various places along the borders of the moor and indicate the old line of beach. A curious confirmation of this geological fact is afforded by the presence—the one on Shapwick Heath, and the other near Glastonbury—of two plants (the *Rumex maritimus* and the *Vicia lutea*) which are shore plants, but which have until recently maintained their places as remains of the ancient marine flora, showing the retreat of the sea. The *Vicia lutea* has, I believe, recently succumbed in this interesting locality to the British collector. The description of Glastonbury as the Isle of Avalon, and the account of the bringing of the body of King Arthur from Tintagel to its resting-place at Glastonbury, are confirmations from tradition of the same fact.

Then a change came over the district, apparently by the formation of barriers of sand or mud along what is now the shore of the Bristol Channel, and along the sides of the overflowing rivers, and in that way the sea-water was shut out, and a depressed region left with a mud surface; on this there arose a forest of oak, ash, and yew, then the water stagnated, and in it the Sphagnum grew, and gradually filled it up, killing the growth of trees on the surface of the land, but leaving, down to historic times, spaces of fresh water from which the Abbots of Glastonbury formed their great fishing lake at Meare, by the side of which they erected the beautiful manor house and fish house which still remain. When the Romans occupied this part of England, they not only used the Burtle beds for plastic clay, but used the peat in their kilns, and the remains of the road which they constructed across the moor are now found some six feet below the present surface. In like manner, a pathway exists across part of the moor near Westhay consisting of slabs of birch, and perhaps alder laid crosswise, so as to form a kind of corduroy road. This has been found in one place at a depth of seven, at another of two feet only beneath the surface. The road bears the name of the Abbot's path, or way, and it may well, I think, have been a way by which the monks of Glastonbury passed from their abbey by way of Meare to Burtle, where they appear to have had a chapel which they served. Now, as I have already said, the system of drainage is so complete that the peat, when once cut, is not reproduced (though the lower soil is said to have a remarkable power of expansion and rises often to the old level), and the Sphagnum is to be found rarely, if at all, on many parts of the moor.

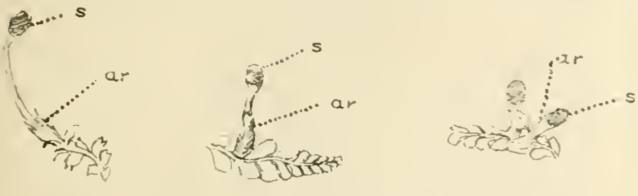
To the intimate structure of the Turf Moss are thus to be attributed great results in the history of the world. To look at our own island alone, but for it the primeval forests that once covered the land might still be standing; but for it large tracts of land would still be lake and mere; but for it every freshet in a Highland river would be a flood; without it we should have had no Mosses on the confines of England and Scotland, and where would have been the border warfare and the border minstrelsy? where the Moss hags in which the hunted Covenanters sought for shelter and freedom of worship? To come southward, by force of its growth, the broad meadows of Somerset have been built up, and the dark waters on which the mysterious barge bore the dead Arthur from Tintagel to Avalon have been turned into the green pastures of Glastonbury and Meare and the battle-field of Sedgmoor.

Hepaticeæ. If my reader will once again refer to table A, he will see that the Hepaticeæ, the lowest group of Mosses, when that word is used in its wider signification, yet remain for some little notice. I am afraid that most people slight them greatly, and feel little inclination to examine them; and yet they possess a beauty of their own—a great diversity of form, and points of great interest and importance to the botanist.

Popularly these little plants would probably be considered Mosses, and it may be hard to say whether or no they deserve the name, and so botanists use two Latin words for the one English one, and allow them to be Muscineæ, though not Musci.

In the springtime, anyone who carefully looks may find,

in woods or on shaded banks, little plants, in such forms as those shown in Figs. 34, 35, and 36. These show two



FIGS. 34, 35, 36.—*Jungermanniæ.* *ar*, archegones; *s*, sporangium. After nature.—A.F.

species of Jungermannia—of the family Jungermanniaceæ of my table A. The genus bears a somewhat uncouth name, and one wishes it had a pleasanter one than this, which was bestowed upon it in honour of a worthy German botanist. It has been said that the names of the Mexican kings before the conquest rob them of the fame which their merits deserve; and so long as these little plants bear this name, I see no hope that they should ever attain popularity.

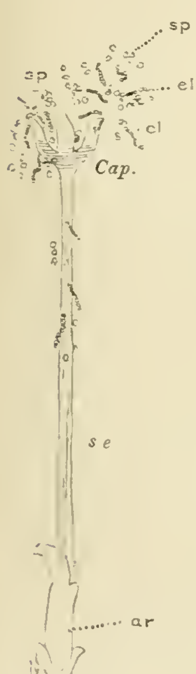


FIG. 37.—Part of a *Jungermannia*, magnified; *ar*, archegone; *se*, seta or stalk; *cap*, capsule of four leaves opening and emitting *sp*, spores and *el*, elaters. After nature. —A.F.

That these little plants have great likeness to ordinary Mosses must be conceded, but the student who looks at them attentively will soon see something different about their general appearance. This is partly due to the peculiar way in which the leaves are inserted—the lines of attachment of the leaves to the stem generally forming an angle like a V. But other differences will be apparent from the figures. The sporangium, as in the Mosses, takes its rise from an archegone. In the Mosses this organ is concealed in modified leaves, and is generally speaking not easy to detect. In these Jungermanniaceæ the archegones form conspicuous green organs (*ar*) in which, in the earlier stages, the dark-coloured sporangia appear to rest (Fig. 36, *s*). They are subsequently raised on a seta or stalk, generally of a perfectly white cellular structure. The sporangium is not divided into capsule and lid, but opens as shown in Fig. 37, by the springing backward of the four leaves which constitute it, and emits large quantities of spores (*sp*), interspersed with very curious structures, known as elaters (*el*). These elaters, with some attached spores, are seen more highly magnified in Fig. 38. The elaters are endless threads, twisted into spirals, and when liberated the spirals rapidly twist and twirl about, and in so doing, aid in dispersing the spores. The spectacle of an opening sporangium of a Jungermannia is a very interesting one.

It is remarkable that in the true Mosses, with their much more highly organized capsules and spore cases, these elaters have disappeared.

If the patience of my reader hold out, I will ask him, for the last time, to refer back to my table A, where he will find that last of all in the series come the Marchantiacæ, so named from the best known species of the group, the *Marchantia polymorpha*, a plant so remarkable and so worthy of full consideration, that I fear that if I embarked on it I should weary my reader beyond endurance, so I leave it, at least for the present.

Distribution in Time.—It will be interesting now to enquire how long the present Moss flora of England has existed. How far back can we carry our knowledge of the existence on the world's surface of these delicate organisms? It is evident that

FIG. 38.—Spores and elaters of *Jungermannia* magnified. *sp*, spores; *el*, elaters. After nature. —A.F.

they have had but a small chance of leaving evidence of their existence as fossil remains, because whilst the strong, almost wiry, vessels of the ferns have a great power of resisting decay and so getting preserved, the delicate cellular structure of the Mosses offers little or no resistance to that process. Hence it is that the fossil remains of Mosses are not very numerous, or for the most part very ancient. Yet we have some materials to answer the enquiry. Three ancient collections of Mosses enable us to throw some light upon it. In an interglacial bed near Crofthead, in Renfrewshire, eleven species of Moss were discovered, and with one possible exception all are well-defined British species of the present day. If we take Mr. Wallace's chronology, and hold that 80,000 years have passed since the Glacial epoch disappeared, and 200,000 years since the Glacial epoch was at its maximum, we may perhaps give from 100,000 to 150,000 years for the age of this little collection. Out of the eleven Mosses discovered, seven belong to the genus Hypnum, or the family Hypnaceæ. This collection, then, is evidence, so far as it goes, (1) that the existing Moss flora is as old as the interglacial epoch; (2) that the Hypnaceæ were as dominant then as now; and (3) that the specific forms have remained constant since that epoch.

Another collection of fourteen Mosses has been discovered in a drift in the Clyde valley above the Boulder drift, and tends to confirm the previous conclusions; as all the species are existing, all now inhabit the valley of the Clyde, and the Hypnaceæ are still predominant, though not in so great a proportion as in the Renfrewshire bed.

A third collection has been found at Hoxne, in Suffolk, in a lacustrine deposit, probably resting in a hollow in the boulder clay. Together with phanerogams of an arctic habit, have been found the remains of ten Mosses, which are described by Mr. Mitten as looking "like a lot of bits drifted down a mountain stream." They are all still dwellers in our island, and exhibit, like the other collections, a preponderance of the family of Hypnaceæ.

But we can give some evidence of more ancient date. Heer inferred the existence of the Mosses in the Liassic period from the presence of remains of a group of small Coleoptera, the existing members of which now live amongst Mosses—an inference which seems not very strong. But recently the remains of a Moss have been found in the carboniferous strata at Commentry, in France. It appears to be closely allied to the extant Polytrichum, the most highly-developed genus of Mosses; so that we have here a phenomenon like that which occurs in reference to the Equisetacæ and Lycopodiaceæ, viz., that the earliest fossil species known belong to very highly-developed forms of the group.

Distribution in Space.—If we turn, now, from the distribution of this family in time to its distribution in space, we shall observe some curious phenomena. Of our English Mosses some are of almost world-wide distribution; some are found here and in spots far removed from our shores; some are believed to be peculiar to our island. One observation should be borne in mind in considering the following statement, viz., that Mosses are often small and inconspicuous plants; that they are often neglected where flowering plants have been collected and studied; and that consequently the statements as to their non-existence must always be accepted with this proviso, that they may mean the non-existence or the non-discovery, or non-observation of the plant in question.

Of our Mosses I have said that some are cosmopolitan in their extension. Our common *Funaria hygrometrica*, our *Bartramia pomiformis*, and, amongst the Hepaticæ, our Marchantia are a few amongst many that are denizens alike of the Old and of the New World. Of the New Zealand Mosses about one-fifth are British, or at least European, species; of the Tasmanian Mosses about one-third.

But whilst there are instances of this wide dispersion, there are instances too of the opposite kind. Amongst the British Mosses and Hepaticæ (as we learn from Mr. Wallace on the authority of Mr. Mitten) seventeen Mosses and nine Hepaticæ are said to be peculiar to the British Islands, and of these, three genera of Mosses and three of Hepaticæ are also non-European (*i.e.*, not known on the Continent of Europe). The three non-European genera of Mosses have their greatest development in the Andes; the three non-European genera of Hepaticæ have their greatest development in the temperate regions of the southern hemisphere. Let me take one of the Mosses of which the genus is non-European—Streptopogon. This genus is thus distributed: Seven species in the Andes; one in the Himalayas; three in the south temperate zone; one in Sussex. Take, again, one of the Hepaticæ, *Acrobolbus*: its species are confined to New Zealand and the adjoining islands, with one species in Ireland.

There are other noteworthy but isolated facts about the distribution of Mosses to which I may here refer: one is, that the great boulders of the plains of the centre of Germany are found to bear alpine species of Moss as if brought from some distant or elevated region; is this due to the actual transport of the boulders on which they live or to the retreat of the northern flora in the tail of the ice as it retreated northward as the glacial period disappeared? In any event it reminds one of the scattered patches of alpine gentians, which are to be found in the great stretch of moorland south of the Danube, and between that river and the Tyrolese Alps.

I have reason, furthermore, to believe that if the Mosses of some parts of the south-west of England were worked out carefully, northern forms would be found to prevail in a way which would require, if possible, some explanation.

The facility with which the wind can carry the small spores of the Mosses probably accounts in some cases for the wide distribution of the organisms. The very small area occupied by some species suggests great susceptibility to local surroundings.

Conclusion.—I can cordially recommend the study of the Mosses to any, old or young, who really love Nature: I have found in it a great source of pleasure during the last few years. The tops of walls, the banks of lanes, the slopes of woods, the mountain passes, each inhabited by different classes of Mosses, are as distinct in their vegetation as the oak or elm or beech counties of England, or the pine-clad slopes or the birch groves of the Alps. A square foot, in some situations, will contain a large number of species of different forms and modes of growth. The long arms of the Hypnum may stretch along the ground, whilst the Tortulas raise their spires of rich brown from out rosettes of verdant leaves, and the Bryums with their pendant capsules vie with them in beauty. One stone or bit of boggy land may be a study in colours—greens, browns, reds, greys, and gold—which my pen would fail to describe. A wall-top may show

> "A stubble field, or a canebrake; a marsh
> Of bulrush whitening in the sun."

Another may present a mimic forest, built up of varied forms, as different from one another as were the huge vegetables of the Coal period from our trees. In a word, I find myself, whenever in the country, surrounded by a world of beauty and interest which I only dimly perceived before I entered on the study, though I have never, I hope, been entirely unobservant of things around me. More than ever I can say—

> "In small proportions we just beauties see,
> And in short measures life may perfect be."

"But how shall I begin the study?" some may say. Gather the first moss you come across, examine it with the naked eye, and then with a microscope, and you will have made some advance. If the British Museum be accessible to you, go to the Botanical Department and examine the collection beautifully arranged and exposed in one of the rooms upstairs. But books—you must have books to aid you, and therefore I will suggest a few. Bagnall's "Handbook of Mosses" will, I believe, be found a very useful first book, and is very inexpensive. "The Handbook of Cryptogamic Botany," by Bennett and Murray, will be a very good one with which to begin the study of the organization of the Mosses. Berkeley's "Handbook of British Mosses" may serve as the second book on classification. Wilson's "Bryologia Britannica" is a more advanced book of the same description, and difficult to get. Dr. Braithwaite's "British Moss Flora," which is in course of publication, is a more elaborately illustrated and expensive book. Two works of Schimper's—"Recherches sur les Mousses" and "Entwickelungsgeschichte der Torfmoose"—are admirable, and his "Synopsis Muscorum Europæorum" is very helpful, especially for finding European species not found in Britain. The work of old Dillenius ("Historia Muscorum"), though of course out of date, is very delightful, and contains many excellent plates, worthy of study even in the present day.

One pleasant duty remains to be performed. Figures 4, 5, 6, 9, 10, 11, 15, 17, 21, 22, 23, 24 have been copied from Schimper's "Recherches sur les Mousses"; Figures 16, 27, 28, 29, 30, 31, 32, from the same author's "Entwickelungsgeschichte der Torfmoose"; Figures 7, 8, from Berkeley's "Handbook." For permission to use these sets of figures I beg to thank respectively Messrs. Freutel and Weitz, Mr. Schweizerbart, and Messrs. Lovell Reeve and Co.; nor can I omit to add my thanks to my daughter, Agnes Fry, from whose drawings, partly original and partly copies, most of the foregoing illustrations have been taken.

ELEPHANTS, RECENT AND EXTINCT.

By R. Lydekker, B.A. Cantab.

(*Continued from page 47.*)

We turn now to the grinding or molar teeth, which present far more remarkable peculiarities, and of which a brief account was given in the article on "Teeth and their Variations," already published in Knowledge. So peculiar, indeed, and unique are the structure and mode of succession

of the molar teeth of Elephants, that they are often very imperfectly understood, even by those who have spent half their lives among these animals. For instance, we find

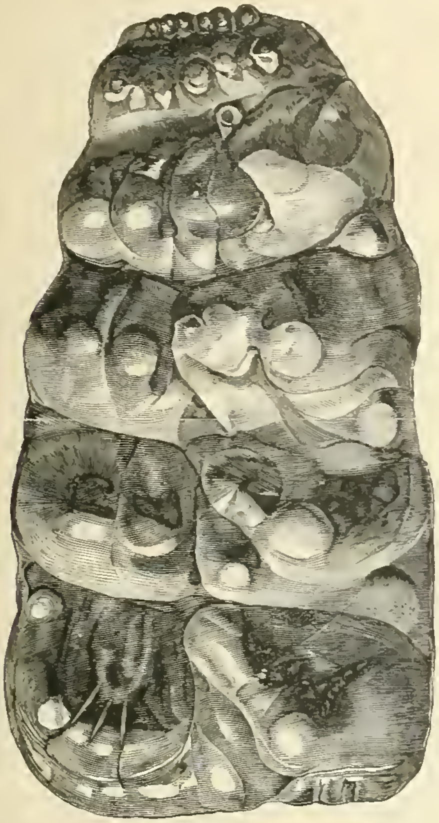


Fig. 3.—Last or Sixth Upper Molar Tooth of a Mastodon; half natural size.

the veteran Elephant hunter, Sir S. Baker, in his "Wild Beasts and their Ways," making the statement in regard to Elephants that "both the Indian and African varieties have only four teeth," whereas, as a matter of fact, every Elephant has, during its whole life, six molars on each side of both the upper and lower jaws. Instead, however, of

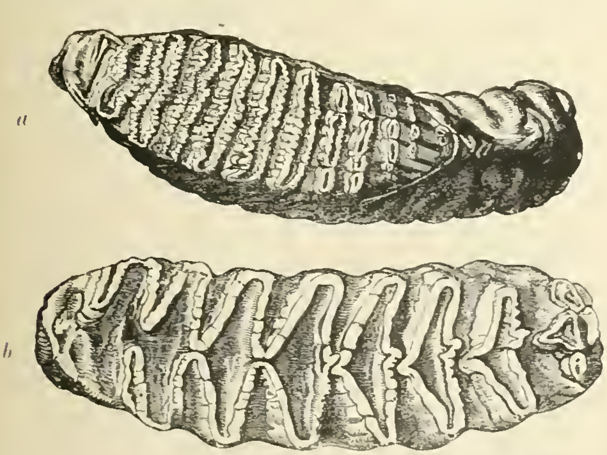


Fig. 4.—Molar teeth of Indian (*a*) and African (*b*) Elephant. In *a* the anterior half is worn, and the remainder unworn. Much reduced. After Owen.

all these teeth being in use at one and the same time, as are those of a cow or a horse, in existing Elephants there are never more than portions of two teeth in use at any one time, although, in the extinct Mastodons, portions of three may co-exist. In all Elephants, the more anterior of these six teeth are smaller and of simpler structure than the hinder ones, and the whole series is protruded in an arc of a circle from back to front, the larger hind teeth being pushed up and gradually coming into use as the small anterior ones are worn away and finally discarded. The hinder ones of these teeth (Fig. 4) are so large that while the front portion is being worn away the back is still bedded in the gum. In the earlier Elephants, or Mastodons, these molar teeth are composed of a series of relatively low and widely separated transverse ridges, more or less completely divided into inner and outer moieties, and with large open valleys between them. Moreover, in all the teeth, except the last, these ridges do not exceed four in number, although in the sixth tooth (Fig. 3) they may be as many as five or six. It was explained in the article on teeth how such a molar tooth might have been derived from the ordinary type of tooth presented to us by the molars of the pig, in which the crown carries four columns, severally placed at its corners. When a tooth like the one represented in Fig. 3 became worn down by use, the enamel coating each of the columns would be cut through, so as to expose a series of more or less trefoil-shaped surfaces of ivory, each surrounded by a ring of the hard enamel. And it will be obvious that a tooth thus constructed of alternations of substances of different degrees of hardness would act as an efficient mill-stone. Elephants do not appear, however, to have been by any means satisfied with this comparatively simple kind of tooth, for as we pass upwards in the geological scale we find that there has been a gradual increasing complexity in the structure of the molar teeth of these animals, this being due to a graduated increase in the height of their transverse ridges, accompanied by an increase in the number of the ridges themselves. There is, indeed, an almost perfect structural gradation, now known to exist between the Mastodon tooth, represented in Fig. 3, to the teeth of true Elephants shown in Fig. 4. Both the latter examples are in a somewhat worn condition, but it will be readily seen that the lozenge-shaped surfaces of ivory, surrounded by enamel, in the tooth of the African Elephant, correspond to the transverse ridges of the Mastodon's tooth. In the true Elephants, however, the open valleys between these ridges (which have now assumed the form of tall, thin, and nearly parallel laminæ) have been completely filled up by a third constituent of the tooth, known as the cement. The grinding surface of the tooth of such an Elephant consequently consists of a solid mass, made up of alternating vertical transverse layers of various substances, arranged in the order of cement, enamel, ivory, enamel, cement; and since each of these constituents differs in hardness, it will be obvious that the millstone-like apparatus is now of a far more efficient type than it was in the Mastodon. Moreover, as the crowns of the molars of the true Elephants are very much taller than are those of the Mastodons, it is evident that they will require a longer period of time before they become worn away, and that they will therefore allow a longer life to their owner. Even, however, among true Elephants there is a considerable amount of difference in regard to the number and narrowness of the component plates of their molar teeth, and it will be seen from Fig. 4, that in this respect the African Elephant departs far less widely from the Mastodon than does its Indian cousin. Since the food of the latter consists to a large extent of boughs and twigs, while that of the former is composed more of juicy leaves, fruits, and roots, the necessity of a more complicated

masticating apparatus in the one than in the other is apparent.

In modern Elephants, the six molar teeth on either side of each jaw are all that are ever developed. This is, however, by no means the case with some of the earlier Elephants, and with most of the Mastodons. In these animals, when the second and third molars became worn out, they were succeeded vertically by much smaller teeth, from which we learn that the first three molars of the modern Elephants really correspond to the milk-teeth of other Mammals, which, as we all know, are succeeded vertically by some of the teeth of the permanent series.* This succession of the teeth shows us, therefore, another point in which Mastodons tend to connect modern Elephants with ordinary Ungulates.

We might go further and enter upon the consideration of some of the structural peculiarities presented by the soft parts of Elephants. Enough has, however, been stated to show that while these animals have preserved a very ancient type of structure in their limbs, they have acquired a very special modification in the structure and mode of succession of their teeth. And it is highly probable that it is owing to this particular specialization that Elephants have survived to our own day, while all the other plantigrade and five-toed primitive Ungulates have completely passed away; while it is almost certain that it is this feature alone which has enabled them to attain the gigantic bulk which forms one of their most striking features. In regard to their evolution perhaps no group shows more clearly than that of the Elephants how exceedingly important is the study of fossils to elucidate the relations of existing animals. Had we only the two living species of Elephants to deal with, we should never have had the least inkling of the manner in which they were related to other Ungulates, imperfect as our knowledge of the relationship still is. Moreover, from the distribution of these two species, it would have been naturally inferred that Elephants were creatures suited solely to tropical or sub-tropical climates. The discovery of frozen carcases of the Mammoth—a species closely related to the Indian Elephant—in the Siberian "tundras" shows, however, that this animal (Fig. 1) was suited to dwell in at least comparatively cold regions, although it is probable that the climate of Siberia was formerly not so rigorous as it is at present. To withstand the cold of these northern regions the Mammoth was protected by a coat of long reddish hair, beneath which was a shorter covering partaking more of the nature of wool. Along the borders of the Arctic Ocean for hundreds of miles Mammoth remains are met with in incredible quantities; and it is still one of the puzzles of geology to account adequately and satisfactorily for the manner in which these creatures perished, and how their bodies were buried beneath the frozen soil before decomposition had begun its work, for it is hardly possible to believe that they lived in a climate so rigorous that their bodies would have been frozen on the surface of the ground immediately after death.

Another rude shock to our common ideas of Elephantine nature is afforded by the extinct Elephants of Malta, which show us that gigantic size is not a necessary concomitant of the group; and that when the area in which a species dwelt was small, the size of the species itself was proportionately reduced. These little Maltese Elephants were very closely allied to the living African species, but whereas "Jumbo" attained eleven feet in height, and wild specimens of the African Elephant may be still larger, the smallest of the Maltese species was scarcely taller than a donkey. So small, indeed, are the bones and teeth of this species exhibited in the Natural History Museum, that it is sometimes difficult to convince people that they really belonged to Elephants at all.

As regards their distribution, Elephants and Mastodons formerly roamed over the whole world with the exception of Australia; true Elephants ranging over the whole of the Northern Hemisphere, while Mastodons extended as far south in the New World as the confines of Patagonia. It is in the north-east of India, Burma, and the islands of the Malayan region that the fossil Elephants connecting the living species with the Mastodons are alone found; and it is thus probable that from these regions the true Elephants migrated westward into Europe and Africa, while the Mammoth in later times crossed from Asia into Alaska by way of Behring Straits. That the Mammoth, which ranged from the Arctic regions to the Alps and Pyrenees, was a contemporary of the primeval hunters of Europe is now a well-established fact, but it appears that throughout the Old World Mastodons had utterly died out before the advent of man. In the New World, however, the continuity between the old and the new fauna was more fully sustained, the Missouri Mastodon having survived well into the human period, so that we have in this survival a good instance of the vast changes that have taken place in the fauna of the globe within what we may metaphorically call the memory of man.

* See article on "Teeth and their Variations."

THEORIES OF GLACIER-MOTION.

By the Rev. H. N. Hutchinson, B.A., F.G.S.

THERE are few subjects in physical geology which have excited more interest than that of the motion of Glaciers. Ice is a very peculiar substance; and some of its properties appear so contradictory to others, that scientific men of high attainments have been greatly puzzled by its behaviour as manifested in Glaciers. Hence when we come to consider how, or why, Glaciers flow down their valleys, we find a great diversity of opinion. Readers of Knowledge may therefore be glad to have a brief account of the different theories of Glacier-motion which have from time to time been brought forward, together with some indication as to their relative merits.

First, with regard to the origin of the ice itself; most of our readers will be aware that Glaciers are fed by the snow-fields above them. So it may almost be said that a Glacier is snow at one end and ice at the other. How then does the change from snow to ice take place? Pressure, as every school-boy knows, will convert a handful of newly-fallen snow into a hard mass, and if the pressure be continued, the hardened snow will become ice. It is partly this property of snow which makes a Glacier, or ice-river possible. When snow has accumulated to a considerable depth, its own weight squeezes down its lower strata; and the underlying portions of snow are finally compacted together until they become true ice. But another cause is at work helping to bring about the same result. By day, when the sun shines upon the snow, or warm air passes over its surface, the surface-layer gets partially melted, the water thus formed trickles down into other snow below, and there solidifying, when night comes and a fall in temperature takes place becomes part of the great mass of ice. Summer and winter act in the same way as day and night, so that much of the winter's snow gets melted and turned to ice. Thus, partly by thawing and freezing, and partly by pressure

(but chiefly by the latter) the snow of the higher regions becomes the ice of the Glacier.

It might be supposed that such an apparently hard and brittle substance as ice would refuse to move downhill, and hence it is by no means easy at first to understand how ice can flow down valleys as it does. The mean daily rate of movement of the *Mer de Glace* (in the centre) during the summer months, is as much as 20 to 27 inches. The question is—how is such a flow to be accounted for ?

In the different theories of Glacier-motion that have from time to time been brought forward, some account for the flow solely by gravitation, ignoring the fact that ice is not a truly rigid body ; others introduce melting, or melting and freezing ; and one brought forward by J. D. Forbes attributes the river-like flow of the ice to a plastic power in the ice itself, as if it were a viscous or semi-viscous substance. These theories may be briefly indicated as follows :—

1. The celebrated *De Saussure*, a pioneer in Alpine work, whose book, "Travels in the Alps," is full of original observations, conceived that the weight of the ice might be sufficient to urge it down the slope of a valley if the sliding motion were aided by water flowing at the bottom. He regarded a Glacier as a rigid mass capable of sliding down an inclined plane just as any solid body might—for instance, as a slate when loosened slides down the roof of a house. There are many objections to this simple theory ; one is that there ought to be accelerated motion, which there is not. Besides, De Saussure was ignorant of certain important facts, to be noticed presently. So we may dismiss this theory.

2. *Hopkins' Theory.*—Mr. Hopkins, a well-known Cambridge mathematical coach, who applied his knowledge to several important geological problems, put forward a theory which may be described as follows : He contended that a Glacier can move along a very slight slope solely by gravitation, owing to the constant dissolution of ice in contact with rock below, and the number of separate fragments into which the Glacier is divided by fissures, so that freedom of motion is imparted to its several parts, somewhat resembling that of an imperfect fluid. His argument was supported by a number of ingenious experiments. He found that ice will move down a very slight slope, even a slope that the eye could not perceive. This theory was very similar to that of De Saussure, only he added to it the idea of a Glacier being broken by frequent fissures into separate pieces. It is needless to say that this theory is out of harmony with the facts.

3. *Charpentier* substituted for De Saussure's sliding theory an ingenious explanation, which may be called "the Dilatation Theory." The most solid ice is always permeable to water, and penetrated by innumerable fissures and capillary tubes, often extremely minute. These imbibe water (due to melting) by day, which freezes during the night, and, of course, expands in the act of congelation. This was supposed to cause a distension of the whole mass, tending to propel the Glacier in the direction of least resistance, namely, forwards. Mr. Hopkins opposed this theory in several able papers. He contended that the distension—if it existed—would tend to act upwards, and increase the thickness of the Glacier, rather than downwards, or in other words, down the valley.

This theory has been demolished in several ways. In the first place, coloured fluids have been injected to see whether the said capillary tubes existed ; but they have never been detected. Again, a Glacier should, on this theory, move faster about the time of sunset, when the freezing of the water must be greatest. But this is not the case.

4. *Mozeley's Theory of Expansion and Contraction.*—Canon Mozeley noticed that the lead on some parts of the roof of Bristol Cathedral kept gradually crawling downwards, tearing up its fastenings in the act. This fact seemed very remarkable, until he explained it by showing that during the day (the temperature being higher) expansion took place, while during the night contraction took place. Both these would take place chiefly in the direction of least resistance, namely, downwards. He then applied this explanation to the downward movement of Glaciers. This theory was ingenious, but like the others, fails to explain all the facts.

5. *Prof. James Thompson's Theory* accounts for Glacier-motion in the following manner :—The freezing point of water is affected by pressure, and relaxation of pressure will cause the water at the bottom of a Glacier to freeze, and a renewal of the pressure will cause it to thaw. His idea is that the pressure due to the weight of a Glacier thaws the ice at the bottom, and that this thawing enables the Glacier to glide downwards (by diminishing friction). Then the relaxation of pressure that follows the down-sliding causes renewed freezing until once more the Glacier's own weight brings about another melting. This theory is also unsatisfactory.

6. *Croll's Molecular Melting Theory.*—This ingenious theory by the late author of "Climate and Time," is rather too subtle. Briefly, he supposed that the ice is melted molecule by molecule, each molecule becoming, for a time, changed into the liquid state, and while liquid descending ; thus a flow of heat was supposed to take place through the whole mass. This is how he accounted for the apparent viscosity of ice in Glaciers.

There are now only two theories left, each of which has powerful advocates now. Some think both are true. The first is—

7. *Tyndall's Regulation Theory.*—Prof. Tyndall believes that a Glacier bends sharp turns by splitting up and freezing together again. His theory is based on Faraday's well-known discovery of regelation, a principle by which when two pieces of melting ice are brought into contact, they will freeze together. The principle of viscosity, so admirably worked out by Forbes, he considers, will only account for a part of the facts. He admits that ice behaves as if it were a viscous substance when it is subjected to *pressure alone*, but when *tension* comes into play, he thinks the analogy with a viscous body ceases. His object is to reconcile the apparent brittleness of ice (for it is decidedly brittle in small blocks) with its power of turning corners, and other facts that seemed contradictory to the idea of brittleness and rigidity. In "Heat and Mode of Motion" he says, "The Glacier widens, bends, and narrows, and its centre moves more quickly than its sides." A viscous mass would, undoubtedly, do the same. But the most delicate experiments on the capacity of ice to yield, to strain —to stretch out like treacle, honey, or tar—have failed to detect this stretching power. "Is there," he asks, "then, any other physical quality to which the power of accommodation possessed by Glacier ice may be referred?" He believes regelation is the required principle, and that the mass of ice in a Glacier moves down the valley by a process of alternate rupture and healing. The gist of the regelation theory is that the ice of Glaciers changes its form and preserves its continuity under *pressure*, which keeps its particles together. But when subjected to *tension*, sooner than stretch it *breaks*, and behaves no longer as a viscous body.

8. *Forbes' Viscous Theory* is opposed by Tyndall, but has many advocates of authority. Principal J. D. Forbes discovered, by a series of measurements, that an ice-stream

moved slower at the sides than at the centre, and faster in the middle, as well as more rapidly at the surface than at the bottom. Consequently he proposed the theory that ice is a plastic substance, capable of yielding to great pressure, and the more so as it approaches the melting point. This theory was not supposed to be irreconcilable with the fact that it will crack under considerable pressure. In small masses this plasticity is not noticeable, but in large masses, and under long-continued pressure, it slowly yields, and will flow like a stiffly viscous fluid. In large masses like a Glacier, this steady, powerful pressure is furnished by the immense weight of superincumbent ice.

Many persons consider that both Tyndall and Forbes' theories are true, and so combine the two; but to the writer it seems that the plasticity of Glacier-ice under great pressure is so well proved as to render the regelation theory almost unnecessary; and according to it, there ought to be more cracks and crevasses all over Glaciers, and not only in their steeper parts.

THE GREAT SUNSPOT AND ITS INFLUENCE.

By E. W. Maunder, F.R.A.S.,

Assistant superintending the Solar and Spectroscopic Departments at the Royal Observatory, Greenwich.

It is a strange circumstance that we seem to have scarcely any observations of Sunspots previous to the invention of the telescope. Only some three or four have come down to us; yet spots large enough to be seen by the unassisted eye are not at all infrequent at the times of special solar activity, and from time to time spots occur which are not only visible, but which are even conspicuous.* The writer well remembers when a boy at school, and not in the least likely to have been on the look-out for such an occurrence, having been surprised by the sight of an intensely black speck on the setting sun. On a more recent occasion, when the Queen was holding the review of the troops who had returned from the Egyptian campaign, the morning was a foggy one, and no dark wedges or smoked glasses were needed to tone down the brilliancy of the sun to the eye. It shone feebly through the fog, a dull red ball, but not, as usually, with unvaried surface; a big black blot was conspicuous upon it, so conspicuous as to attract the attention of the soldiers who were crossing Blackheath on their way from Woolwich to Hyde Park to attend the review, and called forth much comment and speculation from them. It may, therefore, be safely assumed that the object on the sun was something not easily overlooked, and it is hard to understand, if spots of similar magnitude have occurred upon the sun from time to time, during the ages which preceded the days of Fabritius and Galileo, how they escaped notice.

But the great Sunspot of the February just past fairly dwarfed that of November, 1882, to which I have referred above, and whenever the sun was sufficiently dull for the eye to be able to look at it without pain, it was not merely an easy but a conspicuous object. Nor can we wonder that it was so when we come to consider its real dimensions. For on February 13th the spot had an extreme length of 14° of solar longitude, and a breadth of 8·2° of solar latitude, equivalent to 92,000 and 62,000 English miles respectively; whilst the entire group of which it formed the principal part was nearly 25° in length, and 10° in breadth, or in miles 162,000 and 75,000. The area of the great spot on this day reached the astonishing amount of 2940 *millions* of square miles, or reckoning in the smaller spots which clustered so thickly round the central one, the spotted area of the whole group was 3530 millions of square miles. Such an area is all but 18 times that of the entire surface of the terrestrial globe; or to put the matter in another way, some 70 worlds as large as our own could have lain side by side in that immense hollow.

These figures may seem incredible until we recall how vast is the scale upon which the sun itself is constructed. Thus the photographs of the sun taken at Greenwich, one of which is reproduced in the accompanying plate, are on a scale of 8 inches to the solar diameter, a sufficiently large scale to show much detail. Yet a spot with an area of one million square miles would have a diameter of but one hundredth of an inch on such a photograph; practically it would be the smallest object which would be measured; any smaller spot would be quite a negligable quantity. A spot of *only* one million square miles area may be taken as the minutest object that can be satisfactorily dealt with on such a photograph.

We might look at our giant spot from another point of view. Suppose that an object of the same intrinsic brightness as the sun, and of the same apparent size as the great spot, were shining in the midnight sky, how would it appear to us? If we give the sun's "magnitude" as $26\frac{1}{2}$ times brighter than an average first magnitude star, then from such a body as I have supposed we should receive more light than from 130 million stars as bright as Procyon, more light than from 3680 full moons. The planet Uranus is sufficiently bright to be detected by the unaided eye on a clear and steady night by any keen-sighted person, yet the light by which it shines is practically all derived from the sun, which, as seen from Uranus, is not nearly as large as this great spot appeared to us. Even Saturn, bright and readily seen as that planet is, does not receive much more than twice the light from the sun which a region of the sun equal in area to the Sunspot radiates to us.

If, therefore, the Sunspot were absolutely black, its presence on the sun would imply that the light of the sun was diminished by the amount of 3680 full moons, or 130 million stars as bright as Procyon.

Now it is true that the spot was very far from being absolutely black. The greater part of its surface, the penumbra, was dark only by comparison with the greater brightness of the sun; and even the darker portions, the umbra, still radiated a large amount of light. For it has been found perfectly easy to observe the spectrum of the darkest portion of Sunspots, which affords clear proof not only that they radiate some light, but that they radiate a great deal. Still, if we assume the penumbra to be as much as two-thirds as bright as the general surface, and the umbra as one-quarter as bright, we have a loss of light due to the spot of some 1455 full moons; a loss much greater than the total light Neptune receives from the sun, and but little less than Uranus receives. But if we take the figures Sir W. Herschel gives, and regard the brightness of the penumbra as 47 per cent. that of the general disc, and of the nucleus as only 0·7, then our Sunspot becomes responsible for a loss of light equal to 2200 full moons. This is three times as much light as Neptune

---

* The late Mr. John Williams, formerly Assistant Secretary of the Royal Astronomical Society, collected several Chinese accounts of Sunspots, which had been observed by the naked eye. In the *Monthly Notices* for April, 1873, he gave a list of such observations, extending from A.D. 301 to A.D. 1205. Some of the spots were described as "like a hen's egg," "like a duck," "resembling a large date" Possibly our foggy atmosphere, which often enables us to look steadily at the setting and rising, and sometimes at the meridian sun, may account for the fact that spots visible to the naked eye have frequently been observed in England, while, as far as I am aware, the Greeks and Romans did not notice them.—A. C. Ranyard.

receives, considerably more than Uranus receives, and is much more than one-fourth of that which falls upon Saturn.

These figures, of course, represent not the average size of the group, but its greatest size. It would appear to have been quite as large, perhaps even larger, on February 11th and 12th as on February 13th, the date for which the preceding figures have been computed. But it had passed through many changes before it attained so great a development. We cannot trace it back to its formation, for that took place upon the further side of the sun, and it yet remains to be seen whether it will end its course in the visible or the invisible hemisphere. The first time it came under observation was November 15th, 1891, when it was seen at the eastern edge of the sun. It was already a fair-sized spot, with an area of about 470 millions of square miles when first seen, and it increased in size from day to day. At this time, November 19th and 20th, it resembled the general type of the more important spot groups in its appearance, that is to say, it was divided into three principal regions. The largest spot of the group led, then followed a few small scattered spots, and the procession was closed by a large spot of irregular outline. Usually the leading spot in such a group tends to become very well defined in outline and circular in shape, and it increases in size, whilst the rest of the group tend to die out. It was not so with the present group. After passing the central meridian of the sun great changes were seen, in which the following spot assumed the circular form of the first, and the leader broke up into a chain of small spots. This chain, however, conformed to the typical spot history, and before the group had been carried out of sight round the western edge of the sun, it had become reduced to a single circular spot, making with the following spot a pair very similar in appearance and size.

The group was next seen at the eastern edge on December 12th, and remained visible until December 24th, returning into view on January 9th, and disappearing at the western edge for the third time on January 21st. No very noteworthy changes occurred during these two appearances. But when the group reached the east limb for the fourth time, on February 5th, it was altogether changed in appearance and size. It was now five times as large as when it had been last seen on January 21st, and it was to increase still further in size. The distressingly bad weather we had in England during the next week prevented further observations at Greenwich until February 13th, the date upon which the photograph was taken which is reproduced in the accompanying plate. From this date it diminished in area somewhat until February 18th, when it reached the western edge. It returned to our view on March 4th, shrunk in size beyond all recognition. The entire area of the group was now only about 250 millions of square miles, and of the largest spot only 120 millions. It revived a good deal during the following days, but though it attained very important dimensions, and showed several interesting changes, it at no time approached the magnificent proportions it had exhibited during February. It reached the western edge on March 17th, and if it still continues in existence may be expected to reappear again on the eastern border on March 31st or April 1st.

It was, therefore, only during a small portion of its life that the group attained the enormous development shown on our photograph. It happened with this spot, as it often happens with other large groups, that its greatest development took place very nearly at the time when the rotation of the sun brought it nearest to the centre of the disc. The days, therefore, when the actual area was greatest were those when the effect of foreshortening in diminishing the apparent size of the group was least; so that it was seen most nearly in its true proportions just at the time when these were at their maximum.

It was, therefore, only for three or four days that the spot was effective in diminishing the sun's light to the extent we have mentioned. Still that diminution would appear to be so serious in actual amount that even if we regard it as lasting but three days it would seem reasonable to expect that it would make itself felt here. And certain effects were undoubtedly experienced, though perhaps not those which might have been naturally looked for, and though it is not by any means certain that they had any direct connection with a diminished radiation from the sun, it might, perhaps, have been supposed that a loss of light equivalent to that afforded by over 2200 full moons would have seriously affected terrestrial weather. It is possible that it may hereafter be proved that it has such an effect, but up to the present nothing of the kind has been satisfactorily established. And even supposing, what is quite possible, that the diminution of radiation over the spot itself is not compensated by an increased radiation from other parts of the surface, it must be fully borne in mind that the greatest loss due to the spot must be but a very small fraction of the total solar radiation. The difference due to the greater distance of the earth from the sun at one part of its orbit far outweighs it. For the extreme difference due to that cause is one part in fifteen, but the greatest difference which, on the assumptions we have made, we can ascribe to even such a spot as that of February 13th would be no more than one part in 320, or only three times the effect produced by Venus in transit. Any *direct* effect upon the weather, therefore, must, in any case, be of the slightest, the more so that Sunspots are short-lived, and the time during which they present their full face to the earth is necessarily limited to three or four days.

But if the weather shows little or no sign of Sunspot influence, that influence does really exist, and makes itself unmistakably evident; and our February Sunspot did not fail to make its own individual record. Down in the cellars of the Royal Observatory, Greenwich, and the Kew Observatory, and at many other similar institutions all over the world, delicate magnetic needles are kept carefully suspended so as to be free to move at the slightest impulse. The position which such a needle takes up in England is neither a horizontal nor vertical one, nor does it point truly to the geographical north. This deviation from the true north is what is known as the magnetic declination, or the ordinary "variation of the compass." But such a needle does not remain stationary in the position it takes up. Day by day it appears to make an effort to follow the sun as it crosses the sky. The greatest deviation from the mean position towards the west takes place about two o'clock. The needle then begins to move back again, and by about ten o'clock at night has returned to its average position. There is usually but little movement during the night.

This curious daily motion varies in character and extent at different times. Notably it varies with the season of the year, but for our present purpose its variation from year to year is more important. For it has been clearly established that, *when we take average results for successive years*, we shall find that this motion is greater in amplitude and force in strict proportion to the number and size of the spots upon the sun.

That there is this *general* connection between the magnetic variations and Sunspot frequency is admitted. But when we come to the question as to whether individual

spots are able to exercise a special and individual influence, we find there is still a difference of opinion. We may therefore hopefully look to the record of spots like that of November, 1882, or of the February just past, for information on this point, for we naturally expect spots so gigantic in size will be the most powerful in influence.

(To be continued.)

ON THE CONNECTION BETWEEN SUNSPOTS AND MAGNETIC STORMS.

By A. C. Ranyard.

THE lower picture on our Plate represents a large Sunspot, photographed by M. Janssen, on a scale of nearly five feet to the sun's diameter. Its irregular form is characteristic of a period of sunspot maximum disturbance such as we have now entered upon. During such periods the willow-leaf or rice-grain structure of the photosphere becomes rounded and less strikingly distinct than at a period of minimum sunspot development. At all times a mottling of the photosphere is recognizable, produced by the haziness or smudged appearance of the grains or cloudlets in certain areas, while between the hazy areas the brilliant little cloudlets and the small black spaces between them are more distinctly visible, and are seen to be arranged more or less parallel to one another. As a period of sunspot maximum arrives, the character of the whole photosphere seems to alter. The change is not merely confined to the latitudes within 40° of the solar equator, where the spots appear, but extends to the neighbourhood of the solar poles. Similarly the prominences which appear in all heliographic latitudes from the poles to the equator change their character.

We are at present only in a position to observe and collate facts, and we seem to be very far from understanding the great periodic changes going on before us.

There is evidently a close connection between the development of spots on the sun's surface and the swaying of the earth's magnetic axis. More than one popular writer has spoken of this connection as proving that the sun is magnetic; and that solar storms sway its magnetic axis—and, further, that every motion of the great solar magnet is accurately followed by a corresponding motion of the magnetic axis of the planets, which bow and swing, always keeping parallel with the axis of the great central magnet.

But the earth's magnetic axis revolves about the earth's axis of rotation once in twenty-four hours describing a circle amongst the stars of nearly 20° radius. If then, the earth's magnetic axis and the sun's magnetic axis were permanently parallel, we should have to assume that the sun's magnetic axis travels round a line which is not the sun's axis of rotation in a period equal to the earth's period of rotation, which seems highly improbable.

There is considerable difficulty in conceiving of a hot gaseous body like the sun being magnetic. The difficulty occurred to Sir Isaac Newton, who, in a letter written on the 16th of April, 1681, wrote: "Concerning the experiment that a magnet loses its magnetism by heat, some have indeed supposed the sun to be cold, but I believe Mr. Flamsteed is not of this opinion, for they may as well affirm culinary fire to be cold. For we have no argument of its being hot, but that it heats and burns things that approach it, and we have the same argument of the sun being hot. Were we ten times nearer him, no doubt, we should feel him a hundred times hotter, for his light would be a hundred times more constipated, and the experiment of the burning-glass shows that his heat is answerable to the constipation of his light. . . . . The whole body of the sun therefore must be red hot, and consequently void of magnetism, unless we suppose its magnetism of another kind from any we have, which Mr. Flamsteed seems inclined to suppose."

It is possible that though the sun itself may not be magnetic, it may act as a magnetic body because it is surrounded by a magnetic envelope or region where its gaseous constituents are precipitated into solid or liquid magnetic particles. During the past year Professor Dewar has shown that oxygen becomes strongly magnetic when liquefied at a temperature of −180° Cent. The vapours of iron when precipitated in the comparatively hot lower regions of the corona, would also form a cloud of magnetic fog or dust. There is some evidence, in the forms of the coronal streamers seen in the neighbourhood of the sun's poles, that the coronal particles are magnetic, and tend to arrange themselves along lines of force, as if the whole sun had a magnetic axis, nearly but evidently not accurately, coincident with the sun's axis of rotation.

The corona is far from being accurately symmetrical with respect to the sun's axis of rotation; it is denser in parts, and has projecting rays or structures which extend to a great distance from the sun, especially in the sun's equatorial regions. On the above theory we should expect to find the magnetic region similarly unsymmetrical, and a body passing round the sun, near to the plane of the solar equator, would be subject to very unequal disturbance from the magnetic particles of the corona. This seems to tally with the facts observed—for the greatest magnetic storms have generally taken place when a large spot has been seen near to the centre of the sun's disc. We know very little at present as to the connection between the corona and sunspots, or as to how far the corona extends—some of its larger structures may extend as far as the earth's orbit, or as far beyond our orbit as the zodiacal light extends. There is no evidence that large coronal structures exist over large sunspots, but there is evidence of an intimate connection betwen the general development and arrangement of the parts of the corona and the spottiness of the sun's surface, as well as between the development of large prominences and sunspots.

The sudden manner in which these magnetic storms commence seems rather to indicate that the earth plunges into a magnetic or auroral region, than that the magnetic equilibrium of the whole solar system is suddenly disturbed. There is no brewing of a magnetic storm, it breaks out with full violence from its commencement.

Letters.

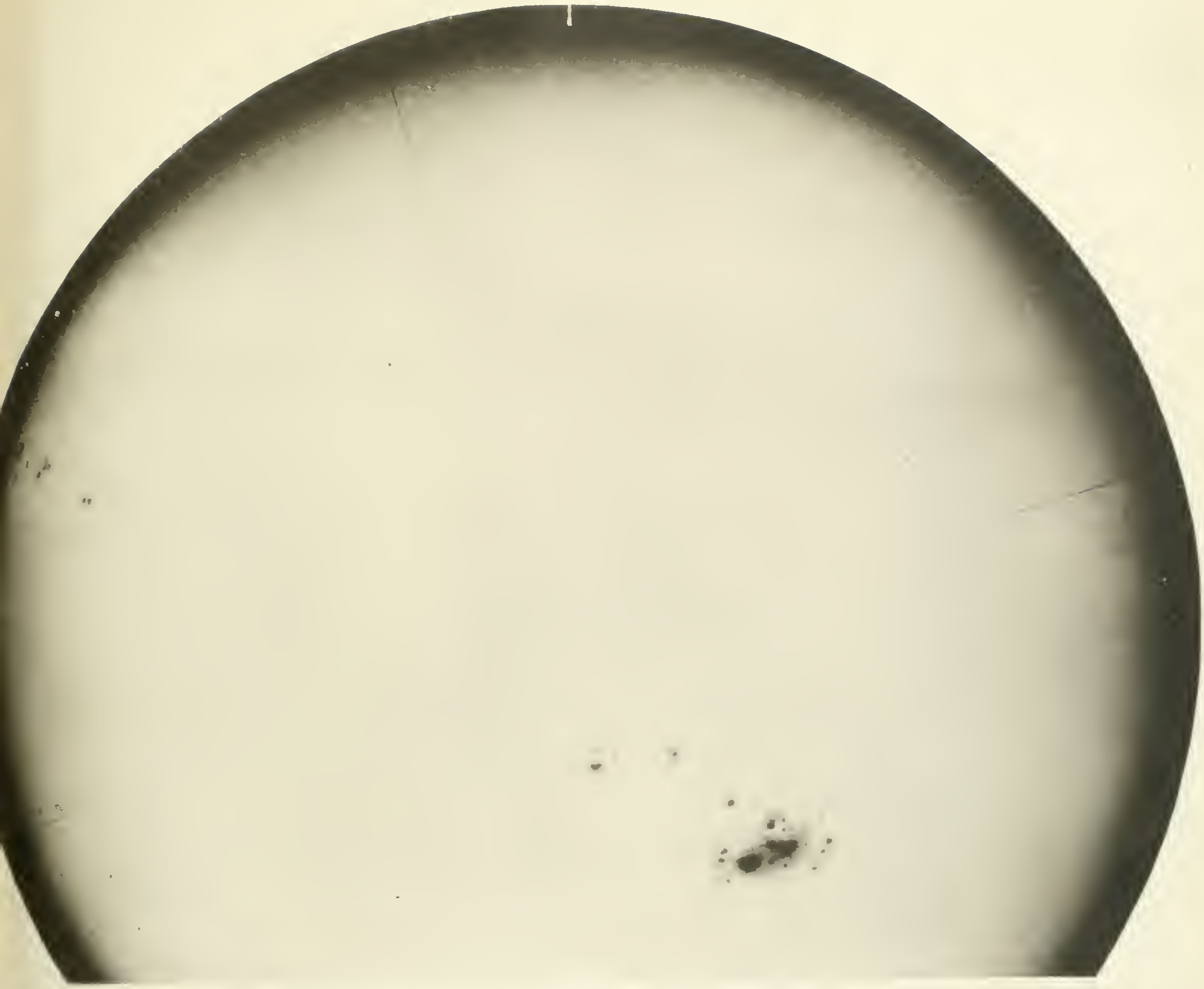
[The Editor does not hold himself responsible for the opinions or statements of correspondents.]

STELLAR SPECTRA.

To the Editor of Knowledge.

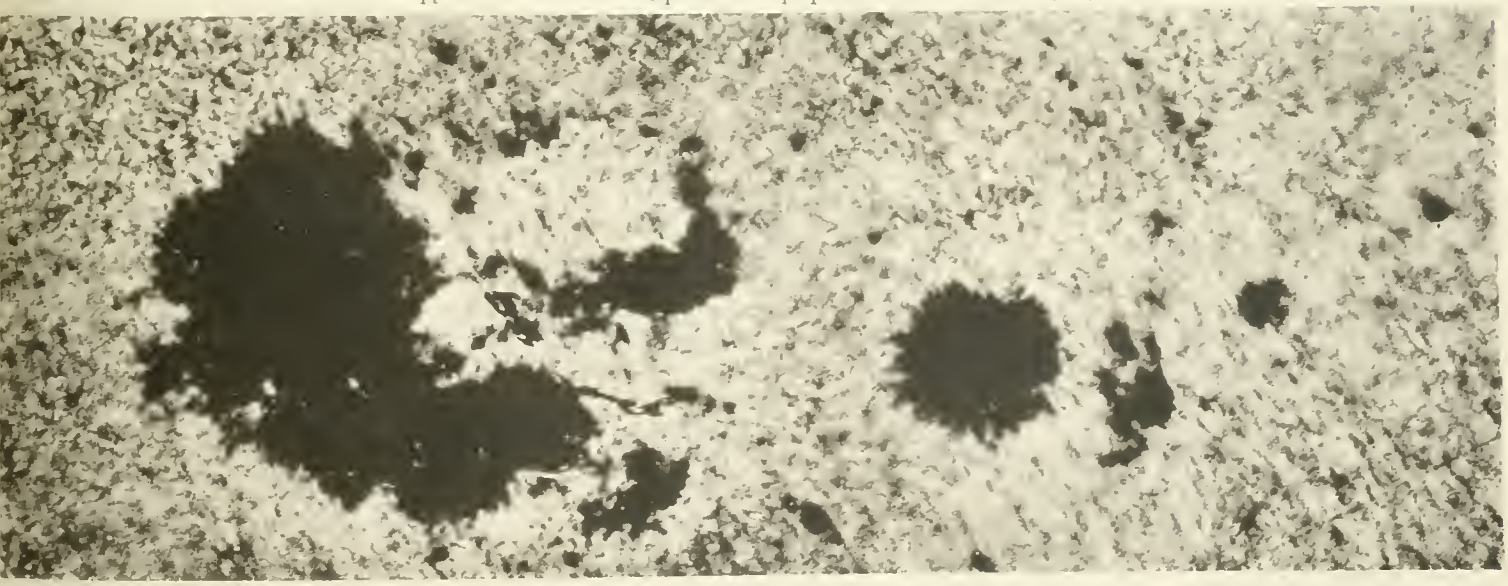
Sir,—With reference to Mr. Monck's remarks on the spectra of binary stars in Knowledge for March, 1892, O Σ 4 seems to be identical with Lalande 220. I find that the star in the Draper catalogue is identical with Lalande 221, which lies about 8′ 25″ north of the binary. Both stars were rated $7\frac{1}{2}$ magnitude by Lalande, and are 8 magnitude in Harding's atlas. They lie closely *south preceding* the $4\frac{1}{2}$ magnitude star σ Andromedæ. The spectrum of O Σ 4 was sent to me by Professor Pickering.

With reference to μ^2 Bootis my authority for its spectrum is Professor Pickering, who kindly sent me the



PHOTOGRAPH OF THE GREAT SPOT ON THE SUN.

Taken at the Royal Observatory, Greenwich, on the 13th February, 1892, at 9h. 47m. 18s. The Sun's axis is placed vertically on the page, with the North Pole uppermost. The wires are parallel and perpendicular to the Earth's axis.



LARGE PHOTOGRAPH OF A SUNSPOT OF MAXIMUM TYPE.

Taken by Dr. JULES JANSSEN, on the 1st June, 1881.

spectra of several binaries by private letter in July, 1890, when I was compiling my catalogue of binary star orbits.

Yours faithfully,

March 18th, 1892. J. E. Gore.

PERIODICAL COMETS DUE IN 1892.

To the Editor of Knowledge.

Sir,—In reference to my short article on this subject in your February number, will you allow me space for a few words to mention that M. Gautier has written a letter to the *Observatory* pointing out that I had overlooked an investigation of his own (published in No. 2656 of the *Astronomische Nachrichten*) on the motions of Tempel's first periodical comet, which shows that, in consequence of great perturbations produced by the action of Jupiter upon it between 1879 and 1885, the duration of its revolution was increased by some months, so that the last return was not due until the autumn of 1885, and another may be expected in the month of April next. That comet was discovered by Tempel at Marseilles, on the 3rd of April, 1867, and was observed at the returns of 1873 and 1879, but has not since been seen. In all those years it passed its perihelion in May, but, according to M. Gautier's calculations, the subsequent lengthening of its periodic time was such that the next perihelion passage probably occurred on the 25th of September, 1885. It was not, however, seen, he remarks, on that occasion "par suite d'un concours de circonstances défavorables." But, as it has kept at a respectful distance from Jupiter since 1885, there is every reason to suppose that it has suffered no further important change in its period, and that it will return to perihelion in the early days of April in the present year.

Yours faithfully,

Blackheath, February 5th, 1892. W. T. Lynn.

ON THE CAUSE OF EARTHQUAKES.

To the Editor of Knowledge.

Dear Sir,—Nearly forty years ago a tremendous explosion occurred at Gateshead, during a great fire. It shook the whole of Newcastle, and was heard at Tynemouth, and ten miles out at sea. Burning rafters were carried high into the air; some of them falling on the other side of the Tyne caused a fire in Newcastle, and one rafter fell in my works, on the Gateshead side of the river, three-quarters of a mile away from the scene of the explosion. I visited the ruins and saw that the *débris* was arranged in the form of a crater, and that inside the crater there was a pool of water thirteen feet deep.

The fire and explosion had been the cause of several deaths, and a very searching enquiry was made at the inquest as to the cause of the explosion. There was a general belief amongst the inhabitants of Newcastle that gunpowder had been stored in the warehouses, contrary to the law, and much indignation was expressed against the warehouse owners, but the evidence of Mr. Hugh Lee Pattison, which I enclose, goes conclusively to show that the explosion was due to the sudden generation of steam. I shall never forget the sensation produced at the inquest when the witness said, "To produce an explosion another element is wanted." He paused, and everybody expected the word "gunpowder" to follow. But he went on, "and that is water," and he ultimately succeeded in convincing the jury that the water they had looked upon as an element of safety was more dangerous than gunpowder.

It seems to me that if water can produce such a tremendous concussion by its sudden conversion into steam when it is confined by warehouse floors, with a few tons of goods upon them, its explosive force would be sufficient to shake the earth, and throw down buildings on the surface, if it were suddenly converted into steam at a great depth. I think that it is pretty generally admitted that volcanic eruptions are always accompanied by a great evolution of steam, which continues to issue and form a white cloud steaming away from the volcanic vent long after the first outburst has ceased; and it seems to me much more probable that the great shaking of the earth's crust, as well as the noises heard during some earthquakes, are produced by such explosions, rather than by the grinding together and slipping of strata along a line of fault, as has been suggested by modern theorists.

Yours faithfully,

George Crawshay.

FROM THE REPORT OF THE INQUEST.

Date of Explosion, October 6th, 1854.

"Hugh Lee Pattinson was then sworn and gave his evidence:—He said he had looked to the contents of the warehouse furnished him by the coroner, and was of opinion that not one of the substances was explosive by itself. He was also of opinion that not one of the substances would become explosive by being roughly mixed together. He thought he could go further and say that hardly any three of them would become explosive. During these two days and this morning, previous to this enquiry, he had made several experiments, not because he felt any philosophical doubt, but to make assurance doubly sure. He had melted nitrate of soda, and when perfectly fluid and red-hot he had poured it into melted brimstone, and there had been produced certainly intense heat, but no explosion. He had introduced into melted nitrate of soda, while red-hot, melted guano, melted zinc and coal tar, that was all the explosive materials, and there had been no explosion; there simply took place deflagration, which, according to Crabbe, meant a gradual sparkling combustion of any substance without violent explosion—a term particularly applied to combustion applied to nitre. It followed, therefore, that the contents of the warehouse alone would not explode. There wanted another element, and that element was water; for we had abundant evidence that when water came into contact with intensely hot and melted saline matter generally a violent explosion took place. This he had also proved by experiment, by introducing a small quantity of water into a crucible and also into a large jar, both containing incandescent nitrate of soda, deflagrating with sulphur, and in each case the vessel was shivered to pieces with a loud explosion, and he produced the pieces in court. Various other experiments of the same nature were made by himself and assistants, all with the same results, and, in support of the same view, he read from the London, Edinburgh, and Dublin 'Philosophical Magazine' for 1850, a paper by Dr. Hare, of Philadelphia, detailing experiments which showed that an explosion similar to the present, which occurred in New York in 1845, had occurred from this cause. Mr. Pattinson went on to say that the fire, the effects of which they were considering, had burned a considerable time before the explosion, and the evidence showed that towards the base of the building, in what was called the vault, there were 47 tons of sulphur spread out on the floor, over which was placed a tarpaulin cover, and upon the top of the cover there were placed 45 tons of nitrate of soda in bags. Now, his opinion was that the sulphur in the vault took fire, which it would do at a temperature of 500 degrees or thereabouts. This would set fire to the bags containing the nitrate, and some of the nitrate would be melted, which, flowing over the burning sulphur, would produce most intense combustion, and this would, in a little time, extend itself to all the sulphur and nitrate in the vault. The large quantity of these substances thus deflagrating in a confined space would necessarily generate an intense heat, more intense, probably, than we could well conceive, and if at this time water in sufficient quantity should find its way into the vault by any means, it would come in contact with the highly incandescent salts, when steam of resistless force would be instantaneously generated, and this would occasion the explosion. It had been imagined that this explosion had been produced by eight tons of gunpowder, nobody imagining that more was required, and nobody talking of any less; but probably four tons of gunpowder would produce the explosion. Now, to compare the force of steam with that of gunpowder they had the following data:—Mr. Robins, who experimented in gunpowder some years ago, found that 27 grains of gunpowder, yielded an explosion of 34·6 cubic inches of permanently elastic gas, at which rate a grain of gunpowder would yield

1·28 cubic inches of gas. 'We find,' says the article, 'Gunpowder,' in the 'Edinburgh Encyclopædia,' 'that 126·7 of gunpowder generates 169·6 cubic inches of gas, from which one grain of gunpowder would yield 1·25 cubic inches of gas. This, he should assume, was correct. Then 253 grains of gunpowder (the weight of a cubic inch of water) would produce 316·25 cubic inches of permanent gas. But one cubic inch of water, expanded into steam, occupied the space of 1728 cubic inches; hence the elastic force of water was to that of gunpowder as 1728 to 316·25 or 5½ to one nearly. It would, therefore, require only 3·66 cwt. of water to do the work of one ton of gunpowder. To do the work of eight tons of gunpowder would be required 29·88 cwts. of water, or 328 gallons, or about six hhds. of 54 gallons each, or 52½ cubic feet of water. Now, there could be no difficulty in imagining that this quantity of water would get into the vault, and cause the explosion.'

Experiments at the Felling Chemical Works.

"On arriving at the extensive chemical works of Mr. Pattinson at the Felling, that gentleman at once proceeded to make experiments, in presence of the coroner, the jury and Captain Du Cane, in support of the evidence he had given on the preceding day. He first caused a metal pot to be inserted in the ground until its top was level with the surface, and having put into it 9 lbs. of nitrate of soda and 6 lbs. of sulphur, he ignited the mass; and then heating it to the highest possible degree of which it was susceptible, he poured into it about a quart of water. The effect was immediate explosion, accompanied by a loud clap, which would have been exceedingly perilous to any person in its immediate vicinity. The experiment was repeated under precisely similar conditions, and attended with a precisely similar result. A trial of the same kind, which had been made before the arrival of the coroner and jury, had been accompanied with the additional effect that the metal vessel, which was the subject of it, had a hole blown through it in two or three different places. A series of experiments was then made under slightly different conditions. The pot, into which the sulphur and nitrate of soda were put, was covered over the top with a large piece of thick metal of considerable weight, and above that again were placed several large pieces of clay and earth. It was deemed necessary to try this experiment in an open field, away from any dwelling-house, and which admitted of the spectators placing themselves at a safe distance from the spot. The materials were then ignited as before, and when in the incandescent state water was poured upon the mass down a spout; but the result was only a comparatively slight explosion, and which scarcely disturbed the iron and clods placed over the mouth of the vessel. Another experiment of the kind was made with the same result. At length, a trial having been made for a third time, but with this difference, that the vessel was covered over the top with another similar vessel, and that the water was poured upon the sulphur and nitrate of soda with greater rapidity than before, by slightly elevating the spout, the effect was to blow up the pot on the top into the air to a height of upwards of 70 feet, accompanied by a loud detonation. With this the coroner and jury seemed convinced that whether or not the premises in Hillgate contained gunpowder, they contained, at all events, elements as certainly explosive, and, perhaps, far more destructive."

N.B.—A large water-pipe inside the building appears to have burst; thus adding to the water from the fire engines.

[There is some difficulty in understanding how without a prior shock, or dislocation, water could in any quantities enter heated cavities in the earth. A little water percolating in would, if converted into steam, tend to drive back and check the entrance of more water from above; and to produce a great explosion a considerable mass of water would need to be admitted rather suddenly to a hotter region. The slipping of strata, or any sudden change along a line of fault might, no doubt, afford a way for such a down rush, but one has to account for the initial movement. If, as is so often assumed, there is an average rise of temperature as we descend within the earth, amounting to 1° Fahrenheit for every 50 to 60 feet, water would be raised to 212° Fahrenheit at a depth of much less than two miles (*i.e.*, somewhere between 7500 and 9500 feet). Water under pressure would glow with a red heat at a depth of ten miles, and cast iron would glow white, and be raised to a temperature which would melt it at the earth's surface, at a depth of 30 miles. But the ordinary assumption with regard to a rapid increase of temperature is founded upon observations made within such mere scratches in the earth's crust, that we cannot safely assume that the temperature will increase uniformly as we proceed to greater depths. Our deepest mines only extend to a depth of a little more than half a mile from the surface, while our deepest borings do not extend to four-fifths of a mile in depth. In entering this mere outer skin of the earth it is found that there are great inequalities in the observed rise of temperature in different regions of the globe, such as certainly would not exist if the increasing temperature were only due to the secular cooling of the interior of the earth from an originally-heated mass. Thus, according to Prof. Judd, at the great boring of Grenelle at Paris, the increase of temperature down to the depth of 740 feet amounted to 1° Fahrenheit for every 50 feet of descent, but from 740 feet down to 1600 feet the rate of increase diminished to 1° for 75 feet of descent. In the deep boring of Sperenberg, near Berlin, which attained the great depth of 4052 feet, the increase of temperature for the first 1900 feet was 1° Fahrenheit for every 55 feet, and for the next 2000 feet it diminished to 1° Fahrenheit for every 62 feet of descent; while in the deep well of Buda-Pesth a decline in temperature was actually found below the depth of 3000 feet. On the other hand, at Monte Massi, in Tuscany, the temperature was found to increase at the rate of 1° Fahrenheit for every 24 feet of descent, while in the Comstock Mine there is a rise of temperature of 1° Fahrenheit for every 45 feet of descent between 1000 and 2000 feet from the surface, and a similar rise for every 25 feet at depths below 2000. Such irregular increases of temperature seem to show that there are great local sources of heat, due possibly to chemical changes going on, or more probably to the great crushing and crumpling of strata along lines of weakness, which are always taking place; raising mountain ridges and depressing other regions as the external shell of the earth contracts by secular cooling upon the central mass. It is a fact well known to geologists, that rocks which appear to have been subjected to great movement and contortion also appear to have undergone great chemical change, such as would follow from their being subjected to a high temperature in the presence of water. With such evidence of stress and change going on within the body of the earth, we need not necessarily look to explosions as the cause of the tremors which occasionally agitate the earth's surface.—A. C. Ranyard.]

OUR NEAREST NEIGHBOURS AMONG THE STARS.

To the Editor of Knowledge.

Dear Sir,—The late Mr. Proctor pointed out long ago that the best test of the relative distances of the stars which we possess is their relative proper motions. Not of course that there may not be near stars, whose proper motions are small, because they are moving almost directly towards us or away from us, but that every star, with large proper motion, must be comparatively near, unless we ascribe to it an enormous velocity. This is true, even if the apparent proper motions of these stars are largely due to the sun's motion in space, for it is only on near stars that the effects of this motion will be easily perceptible. When travelling by train, the trees at the end of the next field appear to be moving rapidly, while the distant range of mountains hardly changes its apparent place. In like manner an extremely distant star will appear to be unaffected by the sun's motion, while a near one will be considerably displaced. This conclusion, moreover, is borne out by the results of spectroscopic research. The spectroscope reveals no extraordinary

velocity on the part of some stars as compared with others. This appears not only from the accurate results of Vogel, but from the more extensive but less accurate researches made at Greenwich and elsewhere. I doubt if there is a star observed half a dozen times at Greenwich which gives an average spectroscopic velocity of 80 miles per second, and the average results obtained at Greenwich are confessedly too large. Stars no doubt differ in velocity as well as in other respects, but not to such an extent as to deprive large proper motion of its value as a test of nearness. And an examination of any catalogue of stars, with large proper motion, leads to the same conclusion, by showing us that their real motions are comparable with the sun's motion in space. Thus, taking M. Bossert's Catalogue of Stars, with proper motions of half a second or upwards annually, and taking the right ascension of the point towards which the sun is moving as 270°, the effect of the sun's motion will be to diminish the right ascension of all stars in the second and third quadrants, and to increase the right ascension of all stars in the first and fourth quadrants. As a matter of fact I find that three-fourths of the stars in the second and third quadrants are moving in the direction of diminishing right ascension, and three-fourths of those in the first and fourth quadrants are moving in the direction of increasing right ascension. This is just the result which would be obtained if the sun's motion in right ascension was equal to the average motion of the stars in question. The effect in declination is also manifest, though not quite so well marked, and the result appears to be that the stars in M. Bossert's Catalogue are not on the average moving with much greater velocity than the sun.

But when we examine the spectra of these fast-moving stars, one important fact immediately occurs to us. Over the whole sky the numbers of stars with spectra of the first type (Sirian stars) and of the second type (Solar stars) are about equal; but when we examine M. Bossert's Catalogue of Stars with large proper motion, we find that there are about eight Solar stars to one Sirian. (Stars with other types of spectrum are almost wholly absent.) The sun thus appears to be one of a class or cluster of stars whose spectra are of a type similar to its own. And we begin to see, too, why the brightness or magnitude of a star affords so doubtful an index to its distance from us. *Ceteris paribus*, Sirian stars appear to be brighter than Solar stars, and a classification according to brightness or magnitude will prove to a large extent misleading so long as we omit the consideration of the spectra of the stars. The relative effect of the light of two stars on the eye is often widely different from the relative effect on the photographic plate, and a mere change in the structure of the eye might revolutionize the whole aspect of the heavens. We must, therefore, note the *kind* of light as well as the *quantity* before we found any inference as to distance on it.

Another test of distance occurs in the case of binary stars, which is worth examining. If two stars, which are separable by the telescope, complete their revolution in a moderate period, the inference is that they are comparatively near us; otherwise the mass of the system must be enormous. On the other hand, if the angular distance between the stars is not large and yet the revolution is very slow, the pair is probably at a great distance; otherwise the mass must be very small. I accordingly compared Mr. Gore's Catalogue of Orbits of Binary Stars with the Draper Catalogue of Stellar Spectra, and I found that the former catalogue contained 32 Solar stars to 11 Sirians. That this preponderance was due to the comparative nearness of the Solar stars rather than to their large masses, appeared from the fact that many of them had large proper motions, and that in more than one instance a sensible parallax (with the result that the mass was not very large) appeared to be determined by actual measurement. Such stars are α Centauri, 61 Cygni, 40 Eridani, η Cassiopeiæ, and 70 Ophiuchi. It might be thought, perhaps, that Sirian stars were, for some reason, less likely to enter into binary combinations than Solar stars. But this does not seem to be the case. On comparing the list in the "Handbook of Double Stars" with the Draper Catalogue, I found that the Sirian stars were quite as numerous as the Solar stars—perhaps a little more so. But when we turn to the double stars, whose orbits have been computed, the proportion drops at once from one-half to one-fourth.

The exact limits of this Solar cluster, and the number of stars comprised in it, remain to be determined, but one thing, I think, may be affirmed of it—that it has a northerly drift. Thus, when Professors Boss and Stumpe deduced the position of the apex of the sun's way from stars with large proper motions, they obtained a less northerly position than when they determined it from stars with smaller proper motions.* It might be supposed that this was due to the greater actual velocity of the stars with large proper motions which had the effect of disguising the sun's motion in space, but a comparison of M. Bossert's Catalogue with the more extensive catalogue of Mr. Main does not lead to that conclusion. The effect of the sun's motion in R. A. is more marked in the case of the stars with large proper motions, though that in N. P. D. is less so. The stars with large proper motions are not perhaps faster movers than their compeers, but they have a general drift, which others have not; though whether the more distant stars are moving indifferently in all directions, or have a general drift differing from that of the solar cluster, remains to be determined.

I do not think, however, that all the Solar stars, or even all the bright Solar stars, belong to the cluster or system of which I am speaking. Betelgeuse† and Rigel, for instance, have not only very small proper motions, but judging from the attempts which have been made to determine their parallaxes are nearly as remote as the fainter Sirian stars which abound in the region of Orion. Procyon, Capella, Aldebaran, and Arcturus (in spite of Dr. Elkin's insensible parallax of the latter, in which I have not much faith), on the other hand, probably belong to the Solar cluster. The motions of the stars in this system may be illustrated by supposing that all the elliptic comets which move round the sun were simultaneously visible. They would appear to be moving almost indifferently in every possible direction, yet, taken all together, there would be a general drift in the direction of the sun's motion. Apparent indifference of motion may in this way be combined with a general northerly drift.

Further light, however, may, I think, be thrown upon the structure of the Solar cluster by sub-dividing the Solar stars into two classes, which I propose to designate Capellan and Arcturian respectively, after their most brilliant representatives. The former class includes stars with the spectra denoted by E and F, but more especially F, in the Draper Catalogue. They are, on the whole, somewhat less numerous than the Arcturian stars, but of the 32 Solar binaries for which orbits have been computed, no less than 23 have spectra of the Capellan type, the spectrum in 20 cases being F. They actually outnumber all other binaries put together. Taking Mr. Main's Catalogue of Proper Motions and comparing it with the Draper Catalogue, I found that, roughly speaking, only 24

* See *Old and New Astronomy*, Part XII.

† But the spectrum of Betelgeuse is probably of the third type.

per cent. of the Sirian stars had proper motions, amounting to one-tenth of a second annually (for the Orion stars, Spectrum B of the Draper Catalogue, the proportion falls to 11 per cent.), 36 per cent. of the Arcturian and 65 per cent. of the Capellan. The difference seemed to become more marked as the stars became fainter. It is curious, however, that for stars with very large proper motion —say over one second and a half per annum—the Arcturian type largely predominates. The Arcturians are perhaps the real "runaway stars." The steady-going members of the Solar cluster show no tendency to bolt the course. Such, at least, is the appearance which they present on a first inspection.

I remain, Sir, truly yours,

W. H. S. Monck.

THE CHEMICAL RESEARCHES OF JEAN SERVAIS STAS.

By Vaughan Cornish, B.Sc., F.C.S.

IN the last month of last year the chemical world received with profound regret the news of M. Stas's death, at the advanced age of seventy-eight.

The name of Stas has been a household word among chemists for half a century, and his writings, the celebrated *Recherches sur les Lois des Proportions Chimiques*, have come to be regarded as among the canonical books of chemistry. In all that related to the experimental art Stas stood unsurpassed. The marvellous patience with which he matured his methods, and the skilful care with which the final experiments were carried out, stand recorded in his classical memoirs with that clearness and precision of expression characteristic of French scientific writings. Stas's work bore on one subject only, the determination of "atomic weights," with a view more particularly to ascertain if there existed any simple definite relation between the weights of the chemical atoms. In order to explain how this investigation came to be the mission of Stas's life, we must refer to the state of chemical theory in the second decade of the present century. At this time the laws of chemical combination had been formulated and accepted—the laws, viz., which may be epitomized by saying that "chemical elements combine together only in the proportion of their equivalent weights, or in simple multiples of those proportions." Dalton had propounded an explanation of these laws in his "Atomic Theory," according to which chemical combination was due to the union of chemically indivisible particles, the particle or atom of each element having its own particular fixed weight.

Dalton's theory, the next great generalization after Lavoisier's explanation of the phenomena of combustion, was the result of the discovery of definite and simple numerical relations between certain chemical quantities. It was natural that other minds, impressed by Dalton's theory, should seek for other such numerical relations in the hope of fresh discoveries of Nature's laws. In 1815 a paper appeared in Thomson's *Annals of Philosophy* by Dr. Prout, in which he pointed out certain apparent relations between the atomic weights of the elements as then determined. The idea was at once taken up by other chemists, and took shape in the following form, known as *Prout's Hypothesis*:—"The weight of the atom of each element is a simple multiple of the weight of the atom of hydrogen." The observed deviations were referred to errors of experiment, just as the apparent deviations from the laws of chemical combination were referred to experimental error.

It has been the life-work of Stas to investigate both assumptions, and to show that while the laws of chemical combination are rigidly exact the supposition of Prout is unsupported by experimental evidence.

Prout's hypothesis owes its importance in the history of science to the fact that it seemed to restore the old theory of the unity of matter, which appeared to have received its death-blow with the discovery of the chemical *elements*. But if the atom of each element be exactly once, twice or thrice the weight of the atom of hydrogen, then it is reasonable to suppose that the atoms of all elements contain only one kind of matter, and that the hydrogen atoms are the one class of ultimate particles of which all matter is built up. As the art of chemical analysis developed under the hands of the great Swedish chemist, Berzelius, it became evident that Prout's hypothesis was not tenable in its original form. It was revived, however, in a modified shape chiefly owing to the influence of Dumas. In the modified form, the hypothetical unit weight was that of the half-atom of hydrogen. Later on, Dumas was compelled to retreat yet further from the original position, and to take the quarter-atom of hydrogen as the greatest common divisor of the atomic weights. In this modified form the idea of Prout loses much of its interest, since the "quarter atom" of hydrogen is itself an unknown thing. Nevertheless, the idea of the oneness of matter always exerts a certain fascination, and to some minds this unity of matter appears to be almost a logical necessity. Hence the tenacity with which chemists have clung to the belief that apparent discrepancies were due to errors of experiment, rather than to the inaccuracy of Prout's hypothesis.

Stas began his researches on atomic weights with a strong prepossession in favour of the hypothesis. He chose for his determinations such substances as could be prepared in a high state of chemical purity, and worked with large quantities of substance in order to eliminate the effect of errors in weighing. A large number of experiments, which occupied several years, furnished him with extremely accurate values for the relative weights of the atoms of silver and the alkali metals, and of chlorine, bromine, and iodine. Moreover, the variety of methods employed served to eliminate possible systematic errors—errors, that is to say, not due to want of skill in the performing of an experiment, but due to the method itself. Each substance, moreover, was prepared in several different ways and from different natural sources. Not the least remarkable tribute to Stas's skill is the close accordance between the values he obtained for the atomic weights by different processes of determination. The numbers obtained in this first series of researches were closely accordant among themselves, and wholly at variance with those demanded by Prout's hypothesis. Stas concludes his memoir thus: "Prout's hypothesis must be looked upon as a pure delusion; the elements must be considered to be distinct entities, with no relation between their atomic weights."

The accuracy of Stas's work was admitted on all sides, but his conclusions were contested. The criticisms of the Genevese chemist, Marignac, are historically important, having led Stas to his second and more celebrated research. Marignac contended that it was far from being proved that the constituent elements of many chemical compounds were present exactly in the proportion of their atomic weights. It was possible that many chemical compounds contained normally a very small excess of one or other of their constituents. This criticism strikes at the basis of the Atomic Theory, since that theory is founded on the assumption that the laws of chemical

combination are mathematically exact. For half a century the scientific world had accepted the dictum that the laws of chemical combination were *lois mathématiques*, but the original experiments on which these laws were based were far from being models of accuracy. This fact was admitted by Stas, who undertook the laborious task of a re-examination of those laws, with a view to settle by the most exact methods whether these laws were in fact of mathematical exactness, or, like so many physical laws, only *lois limites* or approximate relations. In 1865, five years after the date of his first series of researches, appeared the *Nouvelles Recherches sur les Lois des Proportions Chimiques*. In this work Stas repeated the more important of his former determinations of atomic weights, with additional precautions. He also subjected to the most rigorous tests the laws of definite, constant, and equivalent proportions which had hitherto rested on the comparatively rough experiments of Dalton, Wollaston, and other workers of the early part of the present century. In this great work Stas confirmed, on the one hand, his previous conclusion that Prout's hypothesis was unsupported by experiment, but showed on the other that the laws of chemical combination, hitherto accepted on insufficient data, were, as far as experiment could prove, actual and veritable mathematical laws. It is impossible to over-estimate the benefit conferred upon science by a man who has the courage to devote years of patient labour to the re-examination of points such as this, and the re-investigation of supposed laws which have been accepted on the evidence of insufficient experimental data. Such work is much needed in the chemical world at the present time, when a vast superstructure of theory is being built upon a comparatively small number of approximate experiments with regard to the behaviour of substances in a state of solution.

From the point of view of the working practical chemist the most important aspect of Stas's researches is that relating to the preparation of chemical substances in a state of purity. Since Stas's time chemists have not been satisfied with the approximate purification of substances which in general sufficed the earlier experimenters. The approximate isolation or purification of substances is the first step in a chemical research; the complete purification is the most difficult and the most important part of exact research in the science. Stas's methods of purification have served as a model for all subsequent experimenters. In order to give a general idea of the character of his work we will describe a method he adopted for the purification of silver, a substance which is, as he says, the "pivot" of his determinations. Silver is a substance which, as Stas showed, can be obtained in a state of almost perfect purity. The way in which it resists oxidation, and the distinctive character and insolubility of certain of its salts, would lead one to suppose that its complete purification would be very readily effected. That this is not exactly the case will be evident from the following description of Stas's method. In order not to make the description unduly long, we omit the special methods of purifying the *reagents* used in the work. These reagents are water, nitric acid, hydrochloric acid, caustic potash, and milk-sugar. Each of these had to be submitted to special processes of purification, lest their use should introduce foreign substances into the silver.

Coinage silver was taken, and dissolved in very dilute nitric acid. Any gold present is left undissolved. The solution of the nitrate is evaporated to dryness, and heated till no more nitrous fumes are evolved. The salt is then dissolved in a small quantity of water. On filtering, any platinum present is left behind. The filtrate is then diluted with about thirty times its volume of water and an excess of hydrochloric acid added. All the silver is then precipitated or thrown down in the form of the insoluble chloride of silver. Any copper and iron present remain in solution. The liquid is poured off and the precipitate washed, first with dilute hydrochloric acid and then with water, till the washing appears to be pure water containing no trace of copper or of hydrochloric acid. This washing of a large quantity of a precipitate is a very lengthy and tedious operation, requiring days or weeks, according to the quantity of the precipitate. The washing is effected in this case by shaking up the precipitate with water in a stoppered flask, allowing the precipitate to settle, and pouring off the liquid. All the operations with chloride of silver were carried out in a room lighted by artificial light, since daylight, as is well known, effects a chemical change in the composition of chloride of silver. The chloride of silver, purified as above, is brought on to a cloth (previously washed with hydrochloric acid) and the water squeezed out. After drying, the silver chloride is pounded fine in a mortar, and *reduced* to the metallic state by warming for forty-eight hours with a solution of caustic potash and milk sugar (both carefully purified). The finely-divided metal is then fused, with special precautions to prevent access of impurities. By this process Stas hoped to obtain an ingot of perfectly pure silver, but found that, besides very slight traces of other substances, there remained an appreciable quantity, 2 parts in 100,000, of silica. Experience convinced Stas that no substance can be obtained absolutely pure except by distillation. He therefore subjected the silver obtained as above to the process of distillation from one cavity to another in a hollowed block of quicklime, made from white marble. The cavity having been previously heated by the oxy-hydrogen flame, in order to drive off any volatile substances such as soda, the silver was placed in the cavity and fused. No scum appeared on the surface, showing the absence of certain impurities such as iron, which under these circumstances would form a slag. The heat from the oxy-hydrogen flame was then increased till the metal began to boil. The vapour had at first a strong yellow tinge, showing that sodium was still present.

This, however, soon disappeared, the vapour of the silver showing no colour beyond a faint blue tinge. The absence of any green tint showed that the substance was free from copper. The metal having completely distilled into the second cavity, or receiver, in the lime block, it was found that absolutely no residue remained, the small quantity of silica, and any similar fixed substance of an acid character, having combined with the lime, and any oxidizable material having been burnt away by the flame of the oxy-hydrogen blow-pipe. By the above process Stas believed that he had obtained silver absolutely pure. Subsequently, however, Dumas showed that silver thus prepared absorbs, after distillation but while still molten, a certain quantity of oxygen which does not combine chemically with the silver but remains "occluded" in the metal. The elaborate precautions adopted by Stas were therefore not successful in obtaining even this well-known and characteristic substance in a state of perfect purity, though he subsequently determined the amount of oxygen present. But the practical chemist owes to Stas a proper appreciation of the difficulties attending the purification of substances, an appreciation of the necessity for taking every means to overcome these difficulties, and a knowledge of methods for the carrying out of this class of work; methods elaborated by Stas thirty years since, and which yet form the basis of many of the recent researches on the determination of atomic weights.

THE LIFE OF AN ANT.—II.

By E. A. Butler.

THE exact nature of the occupations to which a worker Ant devotes itself on attaining maturity, depends somewhat upon the species, but there are, as a rule, certain well-defined heads under which their labours may be classed. Besides the care of the young, which we have already noticed, their chief duties will consist of the construction, enlargement, and repair of the nest, the hunt for provisions, and the defence of the colony. The satisfying of these claims will usually be sufficient to occupy all their time, so that their life becomes one of incessant activity, and their industry is well known and proverbial. Their activity may extend even into the night, for if daylight does not suffice to complete necessary work, it must be continued after dark, and Sir John Lubbock mentions having watched an Ant which worked continuously for nearly sixteen hours, from early morning till late at night, carrying larvæ to the nest.

The character and situation of the nest depend entirely upon the species. The conical or rounded heap of earth or twigs, commonly called an "Ant hill," is not always present, nor does it follow that if Ant hills are made, each distinct hillock represents a separate and independent community. While it very frequently happens that an entire community is located under one roof, such is by no means universally the case, and, therefore, the terms "Ant hill" and "Ants' nest" are not necessarily interchangeable; often one colony will have three or four distinct dwellings, in any of which its members would be at home. Occasionally some communities build much more extensively than this. M. Forel, indeed, cites a case in which a single community of Ants of a very active species (*Formica exsecta*) possessed as many as two hundred dwellings, which were spread over a circular area measuring nearly a quarter of a mile in diameter. So completely had they appropriated this area that, with the exception of a few nests belonging to a very agile kind, no other species of Ant dared show their faces in it. Many and fierce, no doubt, had been the conflicts before this undisputed sway was established, for *Formica exsecta*, though a delicate insect, is a ferocious fighter. Sir John Lubbock thus graphically describes its battles: "They advance in serried masses, but in close quarters they bite right and left, dancing about to avoid being bitten themselves. When fighting with larger species, they spring on to their backs, and then seize them by the neck or by an antenna. They also have the instinct of acting together, three or four seizing an enemy at once, and then pulling different ways, so that she on her part cannot get at any one of her foes. One of them then jumps on her back and cuts, or rather saws, off her head."

While several Ant hills, as we have seen, may belong to one community, it sometimes happens, on the other hand, that a single hillock is shared by two communities, belonging to distinct species, one half being occupied by the one party and the remainder by the other, each maintaining a separate organization, though domiciled under a single roof. Thus the Yellow Ant (*Lasius flavus*), and one of the races of the Red Stinging Ant (*Myrmica scabrinodis*), not unfrequently live side by side in this way, and the distinctness of their establishments is evidenced by the fact that if the nest be disturbed, as for example by the dislodging of a stone, the two parties on rushing out to discover the cause of the disturbance will sometimes come into conflict, and a desperate struggle will take place, each apparently mistaking the other for the authors of the disaster. But there is another way in which a single Ant hill may shelter more than one species. Small and weak species sometimes live in the nests of larger and more powerful ones, completely intermixed with them, though the bond of connection is not always the same. There is a minute Ant called *Stenamma Westwoodii*, which is never found except in the nests of the Wood Ant and an allied species, and so dependent do they seem on their hosts, that when the latter change their abode, their little companions go with them. They are certainly not on terms of hostility to the larger species, which, indeed, take but little notice of them. Not so, however, with another minute Ant called *Solenopsis fugax*; these are real parasites; they make galleries in the walls of their hosts' nests, whence they issue to invade the nurseries of the latter and carry off the young as food. Once within their galleries, they are safe from retribution at the hands of their defrauded hosts, as the latter are too large to get into their tiny burrows. Sir John Lubbock has very aptly compared their depredations to what would be the state of affairs with us, "if we had small dwarfs, about eighteen inches to two feet long, harbouring in the walls of our houses and every now and then carrying off some of our children into their horrid dens."

But further, a smaller and weaker species may be in the nest of a larger one as slaves, or at any rate, helpers. Though the most remarkable of the slave-making species are not British, we have in this country one kind which is addicted to such habits. It is very much like the Wood Ant (*Formica rufa*), and is sometimes called the Red Ant, (*F. sanguinea*). (Fig. 4.) The worker has a red head and thorax, and a black body; it makes raids upon neighbouring nests of different species, capturing those larvæ and pupæ which will produce workers. These, on coming to maturity, finding themselves surrounded only by members of a different species, and having no young of their own to look after, calmly submit to their fate, adapt themselves to their circumstances and set to work to tend the young of their captors. In all Ants, the feeding of the young seems to be the first work to which newly-matured workers devote themselves, and it is certainly the most suitable employment for them until their skins are sufficiently hardened to make them fit to go out on foraging expeditions, when they may have to fight for their own lives or for those of their brethren. The practice of keeping slaves or assistants, of an inferior race, has not advanced in *F. sanguinea* to such an extent as it has in some foreign species, where

Fig. 4.—*Formica sanguinea*. Female. A slave-holding Ant. Magnified 4 diameters.

the captors are entirely dependent, even in the act of feeding, upon the exertions of the slaves. The Red Ant can apparently manage as well without slaves as with them; moreover, it does not seem to be particular about the species to which its helpers belong. Mr. F. Smith, of the British Museum, used to say that he had found the workers of four other kinds in the nests of this slave-owner.

The origin of slave-holding is obscure, but it seems likely that it may have arisen from the capture of pupæ of other species in time of scarcity, to be used as food. Ants in general are quite prepared to use the pupæ of other species for this purpose, and if on occasions of scarcity other nests were plundered and more pupæ carried off than were required for immediate consumption, the residue would probably mature in the nest of their captors, and might then begin to take on themselves the nursing function, to the manifest advantage of the other party, who might thus be induced to bring in more for the same purpose. Summarizing what has been said above, we have shown that a smaller and weaker species of Ant may be associated in the same nest with a larger and more powerful one in any of three ways—as a mere companion or lodger, or even perhaps pet, as a parasite, and as a slave or auxiliary.

The nature of the nest, and, in consequence, the employment of the workers in constructing it, varies with the species. Some, such as the Wood Ant and the *Formica exsecta* referred to above, construct great mounds composed of bits of stick, pine needles, bents of grass, fragments of fern-fronds, &c. These fragments are not in any way fastened together, but simply placed carefully one upon another, in such a way as to form a fairly compact mass, which is firm enough to resist a good deal of pressure. A large mound, of say two feet high, and ten or twelve feet in circumference, must contain many thousands of twigs, and when the number and the careful disposition of these is realised, one can form some notion of the vast amount of labour that its construction represents, all of which falls to the share of the workers. For the collection of the materials, the ground for large distances round the nest is traversed again and again, the army of collectors following certain definite tracks, which, by the trampling of innumerable little feet, in time become distinctly marked roads, and from which, in order to improve them, any obstacles that are not too large nor too firmly fixed are intentionally removed. A suitable fragment having been discovered by any member of the collecting party, the little creature seizes its prize with its jaws in such a way as least to incommode its own movements on its return journey, keeping it, if possible, pointing forward and raised well off the ground. On arriving at the nest it places its load in position, and in doing so a good deal of judgment and discretion must be exercised, for the pile is not simply to be an indiscriminate heap, but is to be traversed through and through with tunnels and passages, large enough to allow free passage to and fro for the inhabitants, and regular enough to allow of pupæ being laid out along them; and openings must be left at different spots on the sides of the mound to serve as entrances to the nest. These also must be so arranged that they can be closed at night by putting up barricades of interlacing fragments, a duty which devolves on sentinel workers. It is nothing short of marvellous that so many hundreds and even thousands of individuals should be able to act in concert, and contribute piece by piece to the raising of so deftly and securely constructed an edifice, without any single presiding genius to lay the plans and see to their execution, and it seems to point to a remarkable degree of intelligence and power of adaptation on the part of the workers, such as to enable them to fit in and utilize to the best advantage fragments of all sorts, sizes, and shapes. It is also a remarkable fact that the symmetry of the nest is always well preserved; its outline is regular, and the slope even on all sides, although the builders, in consequence of their small size as compared with the nest, and their position on its surface when in the act of building, cannot possibly see more than a very small portion of its outline as they deposit and arrange their load.

The above-ground structure just described is not the whole of the nest; there will be tunnels below ground as well, underneath the dome, and the earth from these has to be removed, fragment by fragment, by the jaws of the workers, and carried up above. The growth of the nest, in both its divisions, must, of course, keep pace with the increase of population, which involves ceaseless labours on the part of the architects and builders; while damage to the structure, caused by such accidents as the fall of objects upon it, will call forth the energy and skill of the engineers of the community to effect a speedy repair of the breach. We have thus seen that, in the case of such a species as the Wood Ant, the collection of materials for the construction of the nest involves long and frequent journeys, and no inconsiderable proportion of the time of the labourers must, therefore, be spent in this employment. As its popular name implies, this Ant usually forms its nest in woods, preferring pine plantations, for here there will be abundant material strewed about the ground wherewith to form its conical piles. It is found all over the country. The slave-making Ant, on the other hand, though in appearance very similar, prefers to excavate its galleries in banks, or round the stumps of gorse, and is found chiefly in sandy localities in the southern counties, especially Surrey and Hampshire. *Formica exsecta*, the species referred to above as having such extensive colonies, forms mounds in which bits of fern-fronds, ling, and grass are used. *F. congerens*, an insect very much like the Wood Ant, also makes mounds, and the Rev. Farren White mentions a large one he found on a sloping bank covered with fern, heather, and gorse, on the margins of a stream; it was about a foot high and over eighteen feet in circumference, and had seven entrances.

Our other Ants do not form mounds of this sort, but chiefly excavate the soil, or the trunk of a tree. Some, like *F. cunicularia*, mine under stones, taking advantage of the protection thus afforded. The common *F. fusca*, one of the chief slaves of the Red Ant, delights in banks, especially such as face the south. The margins of the footpaths in our gardens often bear witness to the presence of the Garden Ant (*Lasius niger*), in the little heaps of grains of earth which the workers have laboriously brought out one by one from the soil beneath; this species will, however, sometimes excavate the stumps of trees. The Black Ant (*L. fuliginosus*) usually fixes on the bottom of an old post or the decaying stump of a tree for its abode; it excavates the wood in galleries, the walls of which always become stained black in consequence of its presence. The abodes of the common Yellow Ant (*L. flavus*) are the rounded grassy mounds, generally something under a foot in height, which are so familiar in meadows and heathy places. In this instance the dome is composed of earth, and though this, as well as the ground beneath, is excavated through and through with tunnels, the growth of the turfy grass is not interfered with, and hence great firmness and compactness is imparted to the nest. As the colony multiplies, the dome increases in size by the transference to the outside of the particles obtained from the excavations of the interior, in such a way as to distribute it uniformly and not destroy the symmetry of shape. As the

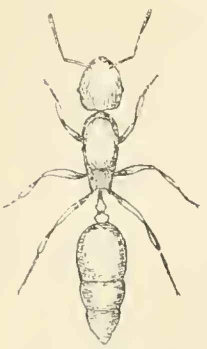


Fig. 5.—Queen of *Leptothorax acervorum*, deprived of wings. A burrower under bark. Magnified 8 diameters.

deposits are gradual, grain by grain, they do not prevent the continued growth of the grass, which is ever extending itself outward, forming an excellent thatch and acting as an efficient watershed in case of rain. The insect can, however, adapt itself to circumstances, and in hilly country where grassy mounds are impossible will avail itself of the protection afforded by loose fragments of rock. The Red Stinging Ants (*Myrmica*) similarly excavate the ground under stones, while a neat little reddish species called *Leptothorax acervorum* (Fig. 5) forms galleries under the bark of trees, especially fir stumps. Lastly may be mentioned a thin and wiry small black species, the workers of which have a squarish head which is streaked with longitudinal furrows; it is called *Tetramorium cæspitum* (Fig. 6), and is found at various places along our south-eastern coasts, seeming for some reason or other to prefer to be near the sea. It also shams death when disturbed, folding up its legs and lying quite still. We have thus seen that the lives of the worker Ants in all these latter species are spared much of the labour which falls to the lot of the mound-builders, and whatever journeys are undertaken will be, not for the accumulation of building material, but only for obtaining food.

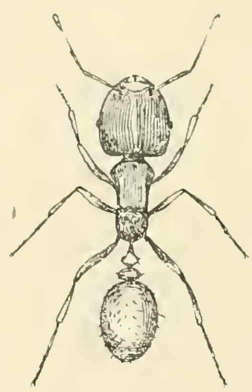


Fig. 6.—*Tetramorium cæspitum.* Worker. A sea-coast Ant. Magnified 10 diameters.

Ants feed chiefly upon insects of various kinds, honey, honey-dew, and fruit, and for a large nest the daily bill of fare must be extremely varied and comprehensive. In nests of only moderate size, the number of individuals will run up to thousands, while of very large ones Sir John Lubbock's opinion is that "perhaps London and Pekin are about the only human cities which can compare with them" as to population. The amount of food that such multitudes would daily need is difficult to conceive, but so far at least as animal food is concerned, the requirements of a large nest would probably demand that many thousands of small insects of different kinds should be slaughtered every day. The hunting and gathering in of this quarry is largely left to individual enterprise, at least amongst the species found in this country, no organized foraging parties being usually employed; possibly, however, certain special individuals may be sometimes told off for the work. The corpse of a larger insect, such as a good-sized beetle or a bumble bee, is an excellent find; if too large and heavy to be dragged to the nest in its entirety, even by the united efforts of a gang of labourers summoned by communications from the original discoverer of the booty, the carcase must be cut up where it lies and carried home piecemeal, an operation not difficult of accomplishment by insects that always carry in their mouths a good pair of scissors in the form of mandibles. The collection of honey-dew leads many Ants to climb trees and plants, and wander over their leaves in search of the deposit, or of the aphides which produce it.

THE FACE OF THE SKY FOR APRIL.

By Herbert Sadler, F.R.A.S.

Large groups of spots and faculæ continue to diversify the solar surface. There will be a total eclipse of the Sun on the evening of the 26th, but as it will not be visible in the northern hemisphere, it need not be further noticed here. The following are conveniently observable times of some Algol-type variables (*cf.* "Face of the Sky" for March). S Cancri.—April 3rd, 11h. 1m. p.m.; April 22nd, 10h. 17m. p.m. δ Libræ.—April 6th, 10h. 12m. p.m.; April 13th, 9h. 46m. p.m.; April 20th, 9h. 10m. p.m.; April 27th, 8h. 54m. p.m. U Coronæ.—April 5th, 11h. 28m. p.m.; April 12th, 9h. 10m. p.m.

Mercury is an evening star during the first of the month, and is well placed for observation during the beginning of April, in the sense of setting some considerable time after the Sun. His brightness, however, has notably diminished since the third week in March. On the 1st he sets at 8h. 30m. p.m., just two hours after the Sun, with a northern declination of 14° 24′, and an apparent diameter of $7\frac{3}{4}''$, $\frac{4}{10}$ths of the disc being illuminated. On the 5th he sets at 8h. 31m. p.m., or 1h. 52m. after sunset, with a northern declination of 15° 39′, and an apparent diameter of $8\frac{1}{2}''$, $\frac{23}{100}$ths of the disc being illuminated. On the 10th he sets at 8h. 16m. p.m., or about $1\frac{1}{2}$ hour after the Sun, with a northern declination of 15° 55′, and an apparent diameter of 10·0″, $\frac{1}{10}$th of the disc being illuminated. After this he rapidly approaches the Sun, being in inferior conjunction with him at 4 p.m. on the 19th. He describes, while visible, a very short arc in Aries, without approaching any naked eye star.

Venus is now a resplendent object in the evening sky, being visible to the naked eye before sunset towards the end of the month. On the 1st she sets at 10h. 51m. p.m., 4h. 20m. after sunset, with a northern declination of 21° 30″, and an apparent diameter of $17\frac{3}{4}''$, $\frac{64}{100}$ths of the disc being illuminated. On the 15th she sets at 11h. 26m. p.m., $4\frac{1}{2}$h. after sunset, with a northern declination of 24° 53″, and an apparent diameter of 20·0″. About $\frac{58}{100}$ths of the disc are then illuminated, the brightness of the planet being about $\frac{2}{3}$rds of what it will be at the beginning of June. On the 30th she sets at 11h. 49m. p.m. (being at her greatest eastern elongation [$45\frac{1}{2}$°] at 5h. a.m. that morning), with a northern declination of 26° 46′, and an apparent diameter of $22\frac{3}{4}''$, just one-half of the disc being illuminated. At about 8h. 30m. p.m. on the 27th the planet will be $\frac{3}{4}'$ north of a $9\frac{1}{4}$ magnitude star. During the month she describes a direct path through nearly the whole of Taurus, being closely south of the Pleiades on the evenings of the 2nd, 3rd, and 4th.

Both Mars and Jupiter are, for the observer's purposes, invisible.

Saturn is an evening star, and is very well situated for observation. He rises on the 1st at 4h. 41m. p.m., with a northern declination of 4° 3′, and an apparent equatorial diameter of 19·2″ (the major axis of the ring system being 44·1″ diameter and the minor 1·0″). On the 30th he rises at 2h. 37m. p.m., with a northern declination of 4° 42′, and an apparent equatorial diameter of 18·6″ (the major axis of the ring system being 42·8″ in diameter and the minor 0·4″). The following phenomena of the satellites may be observed (the times are given to the nearest quarter of an hour). April 7th, $0\frac{1}{4}$h. a.m., Dione, eclipse reappearance. April 9th, $3\frac{1}{2}$h. a.m., Rhea, eclipse reappearance. April 12th, Tethys, eclipse reappearance; 9h. p.m., shadow of Titan in central transit. April 14th, $1\frac{1}{2}$h. a.m., Tethys, eclipse reappearance. April 15th, $10\frac{3}{4}$h. p.m., Tethys, eclipse re-

appearance. April 17th, 8h. P.M., Tethys, eclipse reappearance; 11h. P.M., Dione, eclipse reappearance. April 19th, 4h. P.M., Iapetus, at greatest eastern elongation. April 28th, $8\frac{1}{4}$h. P.M., possible transit of Titan's shadow just skirting limb of planet; 10h. P.M., Dione, eclipse reappearance. On the 6th, at about $0\frac{1}{2}$h. A.M., a $9\frac{1}{4}$ magnitude star may suffer occultation by the planet's southern limb. During April Saturn describes a retrograde path through a portion of Virgo, barren in naked-eye stars.

Uranus is well situated for observation, coming into opposition with the Sun on the 23rd, at a distance from the earth of about 1,627,650,000 miles. He rises on the 1st at 8h. 30m. P.M., with a southern declination of 12° 42′, and an apparent diameter of 3·8″, the apparent star magnitude of the planet being 5·4 in the photometric scale. On the 30th he rises at 6h. 28m. P.M., with a southern declination of 12° 17′, and an apparent diameter of 3·8″. During the month he describes a retrograde path through a barren region of the sky to the W.N.W. of λ Virginis. A map of the path of Uranus is given in the *English Mechanic* for February 12th. Neptune is an evening star, but should be looked for as soon after dark as possible, as he is rapidly approaching the west. On the 1st he sets at 11h. 32m. P.M., with a northern declination of 19° 56′, and an apparent diameter of 2·5″. On the 30th he sets at 9h. 43m. P.M., with a northern declination of 20° 6′. During April he describes a short direct path from a little to the N.W. to a little to the N.E. of ε Tauri.

Shooting stars are fairly plentiful in April, the best marked shower being that of the Lyrids, with a radiant point in R.A. 18h. 0m. +33°. The radiant point rises on the evenings of the 19th and 20th, when the maximum occurs at about 6h. 27m. P.M., and souths at 4h. 8m. A.M.

The Moon enters her first quarter at 6h. 21m. A.M. on the 4th; is full at 6h. 26m. A.M. on the 12th; enters her last quarter at 6h. 0m. A.M. on the 20th; and is new at 9h. $46\frac{1}{2}$m. P.M. on the 26th. She is in apogee at 11·4h. P.M. on the 11th (distance from the earth 252,580 miles); and is in perigee at 9·2h. A.M. on the 26th (distance from the earth 222,090 miles). Her greatest western libration occurs at 6h. 58m. A.M. on the 4th, and her greatest eastern at 10h. 19m. A.M. on the 22nd.

Chess Column.

By C. D. Locock, B.A.Oxon.

All communications for this column should be addressed to the "Chess Editor, *Knowledge Office*," and posted before the 10th of each month.

Solution of Problem in March number.—1. Q to K2, anything. 2. B to R6, and mates next move.

Correct Solution received from Alpha.

Alpha.—Your solution of the four-move problem by 1. Kt to Kt7 goes extremely near. There is, however, one objection. After 1. . . . P to QB4, 2. Kt to B6ch, K to K5; 3. Q to Kt5, P to KB4 saves the mate. 1. B to B3 (threatening B×P, &c.) is the correct key-move.

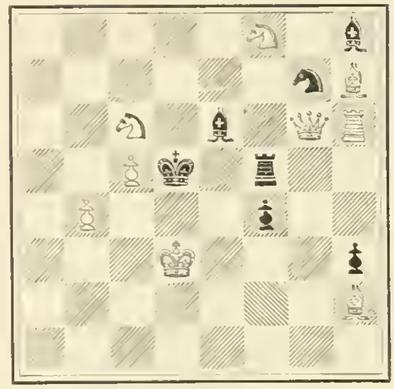
W. T. Hurley.—We hope to adopt your suggestion on future occasions. The winner, like yourself, was a novice in solution tournaments.

N. Pennefather.—At present there is no solution tourney in progress. Should there be another this year, it will be announced in this column. In the March problem, after 1. K×P, P to Kt5; White cannot mate in two more moves.

PROBLEM.

By W. E. B.

Black.



White.

White to play, and mate in two moves.

CHESS INTELLIGENCE.

The match for the championship at Havana resulted in favour of Mr. Steinitz by ten games to eight, five games only being drawn. As soon as the winner began to have recourse to the Close Game, the result was no longer in doubt, for Mr. Tschigorin, as was shown in the previous match, has no chance in this opening.

The following were the results of the various openings in the match:—Evans Gambit, played 8 times by Tschigorin, who won 4, lost 1 and drew 3. Ruy Lopez, played 4 times by Steinitz and once by Tschigorin, Steinitz winning 4 and drawing the other. Two Knights defence, opened 4 times by Steinitz, who won 1 and lost 3. Close Game, played 3 times by Steinitz, who won all 3. In the other three games (Scotch, and Steinitz Gambits), all opened by Tschigorin, an even score was made. If Tschigorin had confined himself to the Evans Gambit, his strongest opening, and Steinitz had relied solely on the Ruy Lopez and Close Game, the score in these openings would have been Steinitz 8, Tschigorin 4, drawn 4; a result which is probably in accordance with the actual relative strength of the two players. The closeness of the match must, therefore, be ascribed to unwillingness on the part of Mr. Steinitz to abandon his unsound variation of the Two Knights Defence.

The result of the Handicap Tournament of the British Chess Club is announced as follows:—1. Mr. C. D. Locock (won 6, drawn 3, lost 0, unplayed 3). 2. Mr. J. L. Cope (won 5, drawn 1, lost 2, unplayed 4). 3. Mr. H. W. Trenchard (won 8, drawn 0, lost 4, unplayed 0); bracketed equal with Mr. Handford (won 4, drawn 2, lost 3, unplayed 3). It will be noticed that Mr. Trenchard was more successful than the other competitors in inducing his opponents to play. But for this, he would probably have come out at least a place higher. The committee could hardly do otherwise than score unplayed games in favour of those competitors who had shown their willingness to play.

The championship of the City of London Chess Club has been won by Mr. Morian, who, in the final tie, won a brilliant game from Dr. Smith.

The National Masters' Tournament of the British Chess Association began at the British Chess Club on March 7th, and was brought to a successful conclusion on the 17th.

There were twelve competitors, who played one game each a day, the evening being reserved for unfinished games. The final score was as follows:—

	Won.	Drawn.	Lost.	Total.
E. Lasker	8	2	1	9
Jas. Mason	6	3	2	7½
R. Loman	6	2	3	7
{ H. E. Bird	6	1	4	6½
{ C. D. Locock	4	5	2	6½
{ R. F. Fenton	3	5	3	5½
{ F. J. Lee	4	3	4	5½
N. Jasnogrodsky	5	0	6	5
L. Van Vliet	3	3	5	4½
J. Mortimer	2	3	6	3½
A. Rumboll...	3	0	8	3
G. H. D. Gossip	1	3	7	2½

The first prize accordingly went to Mr. Lasker, the Berlin champion, and certainly one of the finest players in Europe, perhaps even the greatest Chess genius since Paul Morphy; Mr. Mason, who never quite wins a tournament, owing to his drawing tendencies, took the second prize; and Mr. Loman, the champion of Holland, and last year of the City of London Club and the Divan, the third prize. Messrs. Bird and Locock divide the fourth prize. Mr. Fenton was a little fortunate in some of his drawn games, Mr. Lee being unfortunate in losing to both Messrs. Rumboll and Gossip. Mr. Jasnogrodsky started badly, but afterwards played some fine games. Mr. Van Vliet, except for his victory over Mr. Bird, hardly played up to his reputation; his end-game play was especially careless. Mr. Mortimer, as usual, proved himself formidable to the strongest players, but threw away at least one certain victory.

A match played at Simpson's Divan, between Mr. H. E. Bird and Mr. R. Loman, resulted in a victory for the former by four games to two. The match was practically for the Divan Championship, Mr. Bird having won the last level tournament there, while the previous tournament was won by Mr. Loman.

The result of the National Masters' Tournament, in which Mr. Loman won his game with Mr. Bird, confirms the impression that the two players are, in their different styles, as evenly matched as they could be.

The Amateur Championship of the British Chess Association has been won by Mr. Jones-Bateman. The position of the other competitors is not yet decided.

The following was the fourth game in the recent Championship Match at Havana:—

[RUY LOPEZ.]

WHITE (Steinitz).	BLACK (Tschigorin).
1. P to K4	1. P to K4
2. Kt to KB3	2. Kt to QB3
3. B to Kt5	3. Kt to B3
4. P to Q3	4. P to Q3
5. P to B3	5. P to KKt3
6. QKt to Q2	6. B to Kt2
7. Kt to Bsq	7. Castles
8. B to R4 (*a*)	8. Kt to Q2 (*b*)
9. Kt to K3	9. Kt to B4
10. B to B2	10. Kt to K3
11. P to KR4 (*c*)	11. Kt to K2
12. P to R5	12. P to Q4 (*d*)
13. RP × P	13. BP × P
14. P × P	14. Kt × P
15. Kt × Kt	15. Q × Kt
16. B to Kt3	16. Q to B3 (*e*)
17. Q to K2	17. B to Q2
18. B to K3	18. K to Rsq
19. Castles QR	19. QR to Ksq
20. Q to Bsq (*f*)	20. P to QR4
21. P to Q4	21. P × P
22. Kt × P	22. B × Kt (*g*)
23. R × B	23. Kt × R (*h*)
24. R × Pch	24. K × R
25. Q to Rsqch	25. K to Kt2
26. B to R6ch !	26. K to B3
27. Q to R4ch	27. K to K4
28. Q × Ktch	28. K to B4
29. P to Kt4 (*i*)	Mate.

NOTES.

(*a*) The opening is of the kind made in Germany. In the second game of the present match Mr. Steinitz played Kt to K3 at this point, whereupon Black replied, 8 . P to Q4; 9. Q to B2; P × P; 10. P × P; Kt to Q2, and got a good game.

(*b*) This and the succeeding manœuvres with the Knight take up much valuable time. He might play P to Q4 while he can.

(*c*) Vigorously taking advantage of Black's unusual passiveness.

(*d*) This makes matters worse. He might try R to Ksq and Kt to Bsq, as suggested by Mr. Lee. On his next move he would do better by taking with the RP.

(*e*) In order to prevent Kt to Kt5 by keeping some pressure on White's KKtP. White now proceeds to finish his development before the final attack.

(*f*) A fine retreat, with a view to the sacrifice of the Rook if he gets a chance.

(*g*) If 22 . Kt × Kt; 23. R × Pch, K × R; 24. Q to Rsq ch, &c. The capture made is, however, a mistake. He might play 22 . Q to R3.

(*h*) A blunder which gives Mr. Steinitz his pretty finish. R to K2 was surely better.

(*i*) The whole finish is most artistic, and the game a fine specimen of Mr. Steinitz's style, when he likes to be brilliant.

CORRECTION.—There were one or two misprints in the review of Mr. Freeborough's *Chess Endings* last month. In No. 40, for 6. K to R4 *read* 6. K to R5; and for 7. K to R5 *read* 7. K to R4. In No. 130, for 4, 5, 6, *read* 6, 7, 8. Mr. Freeborough has called our attention to these errors.

CONTENTS OF No. 77.

TERMS OF SUBSCRIPTION.

"KNOWLEDGE" as a Monthly Magazine cannot be registered as a Newspaper for transmission abroad. The Terms of Subscription per annum are therefore as follows:—To any address in the United Kingdom, the Continent, Canada, United States, Egypt, India, and other places in the Postal Union, the Subscription is 6 shillings, including postage; or 1 dollar 50 cents; or 6 marks; or 7 francs 50 cents.
For all places outside the Postal Union, 5 shillings in addition to the postage.

Communications for the Editor and Books for Review should be addressed, Editor, "KNOWLEDGE" Office, 326, High Holborn, W.C.

KNOWLEDGE
AN ILLUSTRATED
MAGAZINE OF SCIENCE
SIMPLY WORDED—EXACTLY DESCRIBED

LONDON: MAY 2, 1892.

CONTENTS.

ON THE ORIGIN OF BINARY STARS.

By T. J. J. See, B.A., B.Lt., B.Sc.

When Sir William Herschel was exploring the sidereal heavens, he found a great number of stars with close companions, which he frequently measured with a view of detecting relative annual parallax. And although in this object he did not succeed, his measurements afterwards led him to a discovery of much greater importance, when he recognised for the first time that some of the double-stars are physical systems in actual revolution. The classic observations of Sir John Herschel greatly extended the list of double-stars, and more recent measurements show that some 600 of the 10,000 objects now enumerated in various catalogues are binary systems in visible orbital motion. Some of these systems are so rapid that during the last hundred years several revolutions have been accomplished, but by far the greater number are so slow that centuries must elapse before their great periods are completed. Sir William Herschel also perceived the very intimate connection between stars and nebulæ, and appealing only to the law of continuity was led to suspect that nebulæ in the course of immense ages develop into stars. Following this line of thought, he divided the whole assemblage of objects into celestial species, and a great impetus was afterwards given to his speculations by Laplace's formulation of the Nebular Hypothesis, based upon phenomena observed in the motions of the planetary system. The epoch-making discoveries following Dr. Huggins' application of the Spectroscope to the study of the heavenly bodies, have at length confirmed the conjectures of Herschel and Laplace, by showing that many of the nebulæ are masses of glowing gas in the process of condensation, and hence it now becomes a matter of great philosophic interest to investigate the process by which nebulæ have developed into stellar systems.

About four years ago the writer proposed to himself to investigate the origin of Binary Stars, and for this purpose collected a table of the orbits of various systems, from which the remarkable fact was discovered that these orbits are very elongated in comparison with the nearly circular orbits of the planets and satellites. It was at once evident that so remarkable and fundamental a difference could not be overlooked in explaining the origin of double-star systems, and the high eccentricities seemed to point with overwhelming probability to the operation of some powerful physical cause which had not left a corresponding impress upon the orbits of the planetary system. Accordingly it was immediately suspected that the cause which had elongated the binary orbits was the secular reaction arising from the tidal friction in the bodies of the stars;* and this hypothesis has been confirmed by subsequent mathematical investigation, in which methods were followed analogous to those employed by Prof. G. H. Darwin in his graphical history of the Lunar-Terrestrial system. As the results of this research seem to throw an entirely new light upon the formation of stellar systems, it may be interesting to show, in an elementary geometrical manner, how the eccentricities have been developed by the secular action of tidal friction, and to point out the probable origin of Binary Stars.

Self-luminous bodies, such as the Sun and double-stars, are certainly in a fluid state (the term *fluid* being used in the most general sense), and there is reason to believe that the viscosity or "stiffness" of the fluid is usually small. Therefore the tides raised in such masses by the attraction of foreign bodies will not be confined to the surface (as in case of the fluid oceans surrounding the nearly rigid Earth), but will extend throughout the whole mass; such tides are termed *bodily* tides, and it is with them that we are here concerned. Now, imagine a double-star system, whose components we shall call respectively Helios and Sol,† each of which is of the same order of mass, and same general physical condition as the Sun. Suppose both stars to be spheroids endowed with rotations which are rapid compared to their period of revolution about one another, in the same direction, and about axes nearly perpendicular to the plane of orbital motion.

Let the system be started with the spheroids at a considerable distance apart, so that the attraction of either upon the other becomes practically the same as if the masses were collected at the centres of gravity, and suppose the orbit given a small eccentricity. Then, since the fluid is more or less viscous, the tides raised in either mass

* The writer had previously seen no intimation that tidal friction could increase the eccentricity, but soon proved it, for the case in which the tides lag as in Fig. 1, only to discover afterwards that a similar result had been reached by Prof. George Darwin several years earlier, though it had not been given any particular prominence, and was apparently but little known.

† These names are chosen to fix the attention upon a system composed of two sun-like bodies, such as we find in double-star systems.

by the attraction of the other will lag, and if the viscosity is small the angle of the lag will be only a few degrees. For simplicity we shall now treat the spheroid Sol as having its mass collected at its centre of gravity, and examine the effects on the eccentricity arising from the tidal reaction of Helios; but it must be remembered that in general the whole effect of tidal friction in the system of stars will depend upon the aggregate effect of the double tidal reaction arising from the rotations of both bodies—a complication that renders the rigorous investigation in general very difficult.

With Sol thus reduced to a weighted point revolving in the plane of the equator and raising tides in Helios, the tidal configuration will be something like that indicated in Fig. 1.

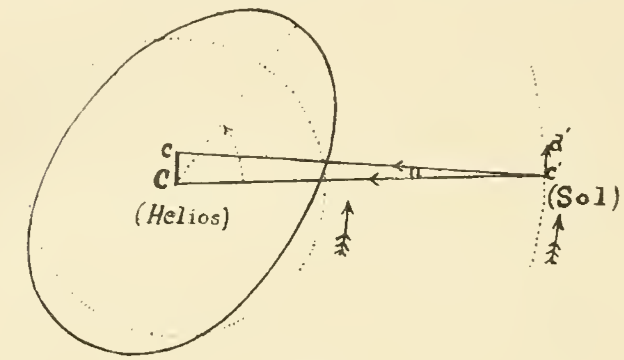


Fig. 1.

In the position of the tidal ellipsoid of Helios shown in the figure the whole attraction on Sol does not pass through the centre of inertia C (about which Helios rotates), but through some point c. The reaction of Sol is equal and opposite, and hence there arises a couple (with arm c C) acting against the rotation of Helios. We may resolve the whole attraction of Helios ($c' c$) into two components, one of which (c' C) passes through the centre of inertia C and produces no effect, as it is counterbalanced by the centrifugal force of the revolving body. The other component ($c' d'$) perpendicular to the radius vector is unbalanced by any opposite force, and hence acting as an accelerating force tends to increase the instantaneous linear velocity, whereby there results an increase in Sol's mean distance.

As the axial rotation of Helios is reduced, Sol is wound off on a spiral whose coils are coincident and very close together. To speak mathematically, the *moment of momentum* of the whole system is *constant*,* and since the reduction of Helios' rotation causes the axial moment of momentum to diminish, it follows that the moment of momentum of orbital motion must augment. In other words, tidal friction transfers moment of momentum of axial rotation to moment of momentum of orbital motion, and hence the mean distance must increase.

With these very brief introductory remarks, let us now examine the changes of the eccentricity of the orbit. In the mathematical works on the tidal theory it is shown that the tide-generating force varies inversely as the cube of the distance of the tide-raising body. The height of the tide, according to the principle of oscillations, varies as the square of the tide-generating force, or inversely as the sixth power of the distance. From Fig. 1 it is easy to see that (for a given lag and given height of the tide) the tangential force varies inversely as the distance.† Therefore the tangential disturbing force varies inversely as the seventh power of the distance of the tide-raising body.

When Sol is in Perihelion the tides are higher (in the inverse ratio of the sixth power of the distance) and the tangential disturbing force is greater than when Sol is in Aphelion, in the inverse ratio of the seventh power of the Perihelion and Aphelion distances. It is well known in the theories of planetary motion that a disturbing acceleration at Perihelion causes the revolving body to swing out further than it would otherwise have done, so that when it comes round to Aphelion the distance is increased. In like manner, an accelerating force at Aphelion increases the Perihelion distance, somewhat as we have roughly shown in Fig. 2. Now, if we consider the tidal frictional component to act instantaneously and only at the apses of the orbit, the effect would be to increase the Perihelion as well as the Aphelion distance, but the latter at such an abnormally rapid rate that the orbit becomes more eccentric.‡

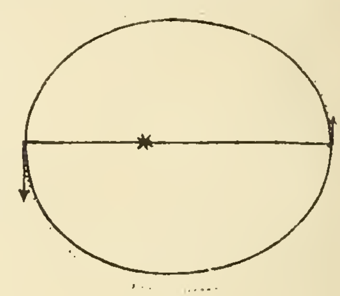


Fig. 2.

If the orbit is not very eccentric similar reasoning to that just employed for the two apses could be applied to other opposite points in the orbit, and the same general result would follow; when, however, the eccentricity is considerable, this method of procedure is not so satisfactory, though while the tides lag, as in Fig. 1, the eccentricity will continue to increase.

We shall now present the effects of tidal friction as the converse of those arising from a resisting medium, and shall determine the law of the density of the medium required to counteract the effects of tidal friction. Let us consider the case in which the orbit has only a moderate eccentricity (say not surpassing 0·3), since practically the whole disturbing force due to the tides in Helios may then be regarded as acting in the tangent to the orbit. When the tides lag (less than 90°, as in Fig. 1), the tangential component is directed forward, and hence tends to accelerate the instantaneous linear velocity; the force arising from a resisting medium is directed continually backward, and hence tends to cause the instantaneous linear velocity to diminish. The two forces are, therefore, oppositely directed, and hence it is evident that if they acted simultaneously the orbit would not undergo the least change either in size or shape, but would be rigorously stable. Now, the resistance encountered at any given point of the orbit depends upon the density of the medium, and is also proportional to the square of the instantaneous linear velocity; but from Kepler's law of equal areas in equal times, it follows that the momentary velocity of the revolving body is inversely as the radius vector. The accelerating force due to tidal friction varies inversely as the seventh power of the distance; therefore, in order to counterbalance this by a retarding force due to resistance we must suppose the density of the medium to

* The *energy* of the system, however, is not constant, but continually diminishing, owing to loss of radiant energy.

† The tangential force is always equal to the whole force acting in the line $c' c$ multiplied by sin ι; sin ι, however (when c C is constant), varies inversely as the radius (vector) ρ.

‡ If the eccentricity is to remain constant the increase must be in the ratio of $(1-e)$ to $(1+e)$; with tidal friction the ratio is more nearly $(1-e)^7$ to $(1+e)^7$, though not rigorously so, except when the eccentricity is very small.

vary inversely as the fifth* power of the distance from the centre. Such a medium would give a resistance that would just annul the changes arising from tidal friction. Now, Laplace has shown† that the action of a resisting medium increasing in density towards the centre, according to any law whatever, causes the major axis and the eccentricity of the orbit of a revolving body to diminish. Therefore, tidal friction must cause the major axis and the eccentricity of the orbit to increase.‡

The mathematical investigation to which we have referred indicates that the double-stars have arisen from double nebulæ,¶ which are certainly in general figures of equilibrium rendered stable by rotation, as is shown by comparison with figures of similar form mathematically established by Professors Poincaré and Darwin. It also appears that the orbits of the double-stars were originally nearly circular, and necessarily so, because of the very slow process by which double nebulæ separate under gravitational contraction and increasing angular velocity of rotation, whereby a division is accomplished closely resembling "fission" among the Protozoans.§ The resulting masses seem to be comparable and often nearly equal, and this result is in accordance with what we find to be the case among the double-stars. In the course of immense ages the nebulæ have condensed into stars, while secular tidal friction in the enormous nebulous masses (for a long time comparatively close together) has expanded the orbits and rendered them very eccentric. The high degree of efficiency of tidal action in the stellar systems results from the large mass-ratios of the component bodies, their state of fluidity, and their enormous absolute masses (frequently several times surpassing that of the Sun) moving at distances such as the larger planets of our own system. It is shown that if the masses were separated as we have supposed the eccentricity of the orbit would at first slightly diminish, then increase until a high maximum is attained, after which it would again diminish (when the stars have become entirely dark).

The stellar orbits are on the average more than twelve times as eccentric as those of the planets and satellites. The mean eccentricity of the 64 orbits now roughly known is 0·48, while the corresponding mean for the orbits of the eight great planets and their twenty satellites is less than 0·0389. The orbit of γ Virginis is known with great precision, and here we have the remarkable eccentricity of 0·9; and the very trustworthy orbit of Sirius, just computed by Dr. Auwers, has the very considerable eccentricity of 0·63. From a number of other orbits whose eccentricities are very well determined the fact seems certain that the double-star orbits are generally highly eccentric, though some few appear to be more circular, in accordance with the theory of tidal evolution under what are perhaps rather abnormal conditions. Therefore we have in the general elongation of the double-star orbits a visible trace of the action of secular tidal friction, which has played so important a part in the evolution of the stellar systems mainly because of the large mass-ratios of the component bodies, and their comparative proximity during immense ages; for it must be remembered that double-stars, now condensed and widely separated, were millions of years ago much closer together and more expanded in volume, and hence the tidal action was then very much greater than at present.

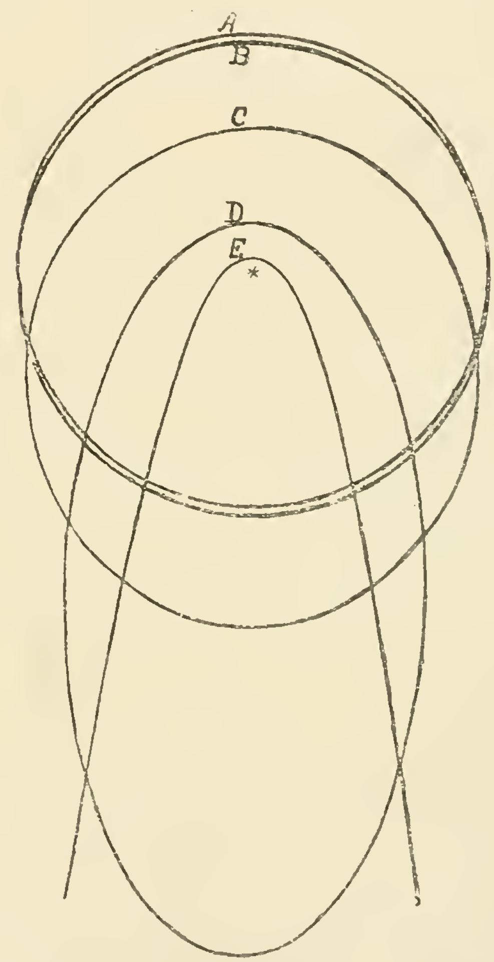


FIG. 3.—A. Circle. B. Mean planetary orbit. C. Mean stellar orbit. D. Orbit of γ Virginis. E. Parabola.

Investigation of the double nebulæ seems to indicate that double-stars were not formed as rings, but as globular masses; and since the process of separation thus disclosed would seem to be the normal form of celestial evolution, some doubt is thrown upon Laplace's theory of ring-formation as applied even in our own complex and remarkable system, composed of a great number of very small planets and satellites moving in nearly circular orbits about central bodies of a much higher order of mass. It seems hardly credible, and yet it is a fact, that our Sun has 750 times the mass of all his attendant bodies combined; the matter of the solar nebula has therefore gone nearly altogether into the Sun, while the planets and satellites are

* If σ be the density of the medium, ρ the radius vector, and κ some constant, then the resistance R varies as $\kappa\,\sigma\,v^2$, but v^2 varies as $\frac{1}{\rho^2}$; therefore R varies as $\frac{\kappa\sigma}{\rho^2}$. The disturbing force F varies as $\frac{\kappa}{\rho^7}$. But R must be made equal to F, hence we must suppose σ varies as $\frac{1}{\rho^5}$. Then $R = F = \frac{\kappa}{\rho^7}$.

† *Mécanique Céleste.* Liv. X., Ch VII, Sec. 18; or Watson's "Theoretical Astronomy," p. 552.

‡ We may add that the increase will usually continue until the rotations of both stars are nearly exhausted, after which the eccentricity will be reduced by the libratory motion of the system, and the orbit will at length become circular. The stars, however, would then perhaps be entirely dark, and hence, if in the immensity of space any such dark rigid double-star systems exist, they cannot be observed. Other relations of rotation and revolution, and various other viscosities, give rise to various other results; but the conclusion above reached is that of chief interest in connection with the great multitude of double-stars hitherto discovered.

¶ It is easy to show that double-stars have not been formed by the approach of separate stars (which would describe hyperbolas or parabolas), and hence the double-star systems must have had a nebulous origin.

§ See also the writer's papers in the *Observatory* for February and March, 1891.

entirely insignificant. On account of the very small masses of the attendant bodies, tidal friction, as Professor G. H. Darwin has shown, has not been of much importance in the solar system, and consequently the orbits of the planets and satellites have in general undergone but little change, compared to the great expansion and elongation experienced by the orbits of double-stars.

The mass of our system is nearly all in the Sun, and hence the mass-distribution is essentially single; the Binary Stars are systems of *double suns*, and the mass-distribution is therefore essentially double. If we look upon the Sun and his attendant bodies as one of the stellar systems, we cannot fail to recognise its very exceptional character. For the system is very complex, and the satellites (except the Moon) are very small compared to the planets, which are in turn equally insignificant compared to the Sun. The orbits are, moreover, in general, nearly circular, as tidal friction has been too unimportant to enlarge materially the (small) eccentricities with which they were originally endowed. By far the greater number of stellar systems appear to be composed of only two stars —two vast suns—but a few systems are made up of three and more rarely four such bodies. We could not discover, with our present optical means, planets such as Jupiter even at the short distance of α Centauri, and hence we cannot affirm that *such* bodies do not revolve elsewhere in the sky, especially around many of the "single" stars. It is doubtful, however, whether there are, in general, many small bodies of a planetary character in the double-star systems, for the simple reason that they could probably not long be preserved where the attraction is so complicated (by two centres of nearly equal importance, varying greatly in distance owing to the high eccentricities of the orbits). Viewed as a stellar system therefore, our system is quite unique, and its development has apparently been radically different from that prevailing among the double-stars, which would seem to be the normal form of celestial evolution.

THE CLASSIFICATION OF THE CHEMICAL ELEMENTS.

By Vaughan Cornish, B.Sc., F.C.S.

THE researches of Stas appeared to show that the connection between the atomic weights, which Prout thought he had discovered, was either unreal or, at all events, not demonstrable. Four years after the publication of Stas's second series of researches, the Russian chemist, Mendelejeff, made known his system of classifying Elements on the basis of atomic weights—a system which has stood the test of experiment, and has pointed the way to many new paths of fruitful research in chemistry.

Chemists had been for some time familiar with the fact that among the Elements are certain "natural families," the members of which bear a general similarity to one another, and show a regular gradation of properties following the increase in atomic weight as we proceed from the lowest to the highest member. Thus Chlorine, Bromine, and Iodine have certain properties in common, known to every tyro in chemistry, which mark them as members of a group or family. Among the members of this family the properties vary in a *continuous* manner with the atomic weight. Thus Chlorine (at. wt. about $35\frac{1}{2}$) is a gas, Bromine (at. wt. about 80) is a liquid, and Iodine (at. wt. about $126\frac{1}{2}$) is a solid, and so in similar gradation with their other characters. Lithium (7), Sodium (23), and Potassium (39) are another natural family, the well-known Alkali metals. Here, again, there is a continuous gradation of properties with rise of atomic weight, Lithia, the oxide of Lithium, being a weaker base than Soda, and Potash being the strongest base of the three oxides.

The members of a natural family are termed *homologous* elements. The connection between the properties and the atomic weight among groups of homologous elements was, as we have said, known before the publication of Mendelejeff's first paper, which appeared in 1869. Mendelejeff, however, by comparing together the members of different groups or families, was led to the discovery of a new and peculiar relation between the weights of the atoms and their properties, a relation which is the basis of the present system of classification of the Elements, known as the *Periodic System*.

Writing the Elements* after Hydrogen (1) in order of atomic weight we have—

Li.	Be.	B.	C.	N.	O.	F.	Na.	Mg.	Al.	Si.	P.	S.	Cl.	K.	Ca.
7	9	11	12	14	16	19	23	24	27	28	31	32	$35\frac{1}{2}$	39	40

and so on. There is found to be a regular gradation of properties with increase of atomic weight from Lithium to Fluorine, which are as opposite in their characters as any two elements with which we are acquainted. Increase of atomic weight from 7 to 19 has continuously diminished the electro-positive or metallic character possessed by Lithium till we reach Fluorine, a non-metal and the most strongly electro-negative element known. But after atomic weight 19 the gradation of properties does not continue; on the contrary, there is a sudden "reversion to type," the next element, Sodium (Na. 23), being strongly metallic in character. It is, as we have already mentioned, a member of the same natural family as Lithium, and in that family stands next to Lithium in order of atomic weight. As we proceed from Sodium, in order of increasing atomic weight, we find once more a gradation from the most strongly marked metallic properties to the most decided non-metallic character. Magnesium (Mg. 24) is a metal as Sodium is, but its oxide is less strongly basic than Soda. Alumina (the oxide of Aluminium, Al. 27) is weakly basic or weakly acidic according to circumstances. Silicon, a non-metallic body, forms a weakly acidic oxide, and Sulphur (S. 32) is not metallic in its physical properties, and forms an oxide which is strongly acidic or acid-forming. The next element, Chlorine (Cl. $35\frac{1}{2}$), is the first homologue of Fluorine, and is a typical non-metallic Element. The Element next following, Potassium (K. 39), is, however, a metal, and forms a strongly basic oxide. It is the third member of the group of alkali metals; so that in passing from Chlorine to Potassium we have the second instance of "reversion to type."

After Potassium, the metallic character again begins to decrease; the next Element, Calcium (Ca. 40) forming an oxide, Lime, which is a weaker base than Potash. Thus, after Potassium as after Sodium, the variation of properties goes on continuously with increase of atomic weight from one element to another for another *period*.

We will not follow these *periodic* variations further, partly on account of limitations of space, partly because the relations become more intricate and more difficult to follow as we proceed to the higher atomic weights. We

* Approximate numbers are, for convenience, given for the atomic weights. The names of the Elements symbolized above are: Lithium (Li.), Beryllium (Be.), Boron (B.), Carbon (C.), Nitrogen (N.), Oxygen (O.), Fluorine (F.), Sodium (Na.), Magnesium (Mg.), Aluminium (Al.), Silicon (Si.), Phosphorus (P.), Sulphur (S.), Chlorine (Cl.), Potassium (K.), and Calcium (Ca.).

have gone far enough, however, to show the peculiar and novel character of the relation which Mendelejeff discovered. Whereas, the properties of the Elements of any one family vary *continuously* with the atomic weight, the properties of all Elements are a *periodic* function of the atomic weight; a certain increase of atomic weight being accompanied by a recurrence of certain properties possessed by an element lower in the scale. This may be made clearer to the eye by writing the above list of 16 elements in a somewhat different way. As there is a return to the metallic character after the sixth element from Lithium, we will begin in our table a second line with Potassium on the left, thus:—

Li. 7, Be. 9, B. 11, C. 12, N.14, O. 16, F. 19,
Na. 23, Mg. 24, Al. 27, Si. 28, P. 31, S. 32, Cl. $35\frac{1}{2}$,
K. 39, Ca. 40, etc.

We see that after a *period* including seven elements, there begins a second period of seven. The difference of atomic weight between the first and eighth Element, between the second and ninth Element, and so on, is in every case, as will be seen on examining the table, sixteen units or very nearly so. Two successive members of any family are separated by six intervening elements, and differ from one another by 16 units of atomic weight. The addition of mass or weight to the chemical atom, from the mass 7 of the Lithium atom to the mass 39 of the Potassium atom, is not accompanied by a continual, unbroken increase of certain properties, but by a *periodic* variation of those properties. The interval from Lithium to Potassium comprises two periods, each of which contains seven elements. The vertical rows contain the natural families (as Lithium, Sodium, Potassium, and Beryllium, Magnesium, Calcium). Previous to Mendelejeff's work relationships could only be clearly traced between members of the same family (or homologous elements). The Periodic system of classification enables us to trace the connection between the *heterologous* elements. Among the following—

	Be. 9,	
Na. 23,	Mg. 24,	Al. 27,
	Ca. 40,	

Sodium (Na.), Magnesium (Mg.), and Aluminium (Al.), are termed heterologous elements, and Beryllium (Be.), Magnesium (Mg.), and Calcium (Ca.) homologous elements.

The elements Sodium, Aluminium, Beryllium, Calcium are termed the four *analogues* of Magnesium. Mendelejeff showed that the properties of any element are completely determined by that of its four analogues. Thus, supposing the properties of the element Magnesium were wholly unknown, those properties could be deduced from the properties of the analogues. Thus, the atomic weight will be the mean of those of the four analogues. Now,

$$\frac{9 + 40 + 23 + 27}{4} = 24\tfrac{3}{4}$$

which gives (approximately) the atomic weight of Magnesium. Again, take the specific gravities. They are as follows:—

Sodium	specific gravity	·97
Aluminium	,,	2·56
Beryllium ...	,,	2·10
Calcium	,,	1·58

Mean of these values = 1·8.

Specific gravity of Magnesium = 1·75.

It will be noticed that although the difference between the weights of neighbouring elements in the horizontal rows (heterologous elements) is not absolutely constant, yet the variations are small, the interval rarely exceeding one or two units in the foregoing table, except in the case of the interval Fluorine—Sodium and Chlorine—Potassium.

At the time when Mendelejeff first drew up his table of the elements, it was found that in several cases the neighbouring heterologous elements did not fall into place, that is to say, did not come into the same vertical row with other members of the same natural family. Thus the element next to Zinc (Zn. 65), which belongs to the same family as Magnesium and comes vertically below it, was followed by Arsenic (As. 75), which thus comes vertically below Aluminium, although its properties are similar to those of Phosphorus not to those of Aluminium.

Thus we have in the second and fourth horizontal rows:

	Na. 23	Mg. 24	Al. 27	Li. 28	P. 31	S. 32	Cl. $35\frac{1}{2}$
and	Cu. 63	Zn. 65	As. 75	Se. 79	Br. 80		

Selenium (Se.) and Bromine (Br.) have properties similar to those of Sulphur and of Chlorine respectively. These facts led to an idea entirely novel in chemistry, that of *gaps* among the elements. Hitherto, the existence of an element with any particular atomic weight and particular properties had been regarded as an isolated and, so to speak, an accidental fact in Nature, but Mendelejeff's generalization introduces the idea of the necessity for the existence of elements with such and such atomic weights, and such and such properties.

It appeared extremely probable that there existed two elements intermediate in atomic weight between Zinc and Arsenic, between which there is an interval of ten units. Supposing two such elements to exist (called provisionally Eka-Aluminium and Eka-Silicon), Mendelejeff arranged the elements in the fourth horizontal row thus:

2nd Row—Na. 23	Mg. 24	Al. 27	Si. 28	P. 31	S. 32	Cl. $35\frac{1}{2}$
4th Row—Cu. 63	Zn. 65	Eka-Al.	Eka-Si.	As. 75	Se. 79	Br. 80

Reasoning from the assumption that the properties of an element are the mean of those of its four analogues, Mendelejeff drew up a table representing the properties of the hypothetical elements Eka-Aluminium and Eka-Silicon. Two elements having the atomic weights required by the position of Eka-Aluminium and Eka-Silicon in the table have since been discovered, and named respectively Gallium and Germanium. Their properties agree very closely with those predicted by Mendelejeff.

This power of prediction of hitherto unobserved elements was an enormous advance in chemical science. The discovery of Gallium holds in the history of chemistry a similar place to the discovery of Neptune in astronomy. The Periodic system of classification enables us not merely to say with every confidence that such and such elements exist though yet unobserved, but it puts us in a position to limit the number of possible, or at all events probable, elements. It enables us to predict with considerable accuracy the properties of chemical compounds before these compounds have been actually investigated, and it has in numberless ways proved of the greatest service to systematic chemistry. The philosophical interest of Mendelejeff's great generalization is not inferior to that of the discovery of the laws of planetary motion. The Russian chemist, like Copernicus and Kepler, has shown the existence of law or order in one of the great departments of Nature's administration. Newton showed that the laws of planetary motion discovered by Kepler were the necessary outcome of the property of universal gravitation. We yet await the discovery of a law which will account for the connection between the weights or masses of the atoms and their properties. Such a discovery would be of surpassing interest. Previous to Mendelejeff,

all that was known of functions dependent on masses derived its origin from Galileo and Newton, and appeared to indicate that such functions always either increase or decrease with the increase of mass, as in the case of the attraction of celestial bodies. The numerical expression of the phenomena was always found to be proportional to the mass, and in no case was an increase in mass followed by a recurrence of properties such as is disclosed by the Periodic law. This is thought to indicate that the true conception of mass, and perhaps also of the mode of action of gravitation, must be sought in the study of the chemical atoms, and of the connection between them by a periodic function of their mass.

THE SYSTEM OF ALGOL.

By Miss A. M. Clerke, *Authoress of "The System of the Stars," and "The History of Astronomy during the 19th Century," &c., &c.*

THE steady advance of exploratory research in the system of Algol promises to furnish one of the most curious and instructive episodes in the history of science. Vague hypothesis, determinate theory, and triumphant verification, have already played their logically sequent parts in the discovery of the eclipsing satellite. Goodricke's conjecture, however, had to wait nearly a century for Pickering's formulization, while this was ratified within a decade by Vogel's disclosure of the anticipated tell-tale spectroscopic effects. Progress has, indeed, of late notably quickened its pace; and we may therefore hope for a prompt and effective application of the Ithuriel-spear of adapted observation to the latest creation of speculative intelligence in the lately organized department of "dark stars."

Since Argelander's time it has been tolerably evident that Algol had other attendants besides the agent in producing its periodical eclipses. For their recurrence was shown by him to be subject to minute irregularities in point of time, and these irregularities are of such a nature as to demand for their explanation the presence of at least one disturbing mass. A highly complex piece of mechanism could plainly be seen to be at work; yet the penetration of its intricacies presented a task so formidable that astronomers of, at any rate, the present generation might well have despaired of its accomplishment. It has, nevertheless, been undertaken by Dr. Chandler; and his labours have been rewarded with an encouraging measure of success.* They have been necessarily of a more or less tentative character, and their result must be looked upon as merely provisional; but there is much reason to suppose that it at least approximates to the truth. It is, moreover, perfectly plain and straightforward; there is nothing of the *obscurum per obscurius* about it; the consequences it involves are definite, and admit of definite verification.

The new and enticing hypothesis now presented for the consideration of astronomers is mainly founded upon certain well-ascertained inequalities in Algol's period of variation. These were shown by Dr. Chandler's discussion some little time since† to be slowly compensatory. They are oscillatory, not progressive. Consistently in advance of their due time down to about the year 1804, the obscurations of the star then began to fall behind it, and the delay had accumulated in 1843 to 165 minutes. A gradual process of restoration thereupon set in, and the normal epoch was reached near the beginning of 1873. It was quickly, however, transcended, for acceleration is still going forward, and is likely to continue operative during some years to come. These irregularities are evidently comprised in a cycle considerably exceeding one hundred years; and for that very reason, it is difficult to account for them on gravitational principles; since a third body, exterior to the close pair, should, in order to produce any marked perturbational effects, revolve much nearer to them than would be consistent with so long a period. Another mode of explanation is, accordingly, resorted to by Dr. Chandler. The varying intervals needed for the transmission of light from different parts of a large orbit described by Algol and its dark satellite round a remote primary, are, in his view, the fundamental cause of the alternate anticipations and retardations in the occurrence of Algol's eclipses. They are, in fact, apparently shifted backwards and forwards in time, just in the same way as are the eclipses of Jupiter's satellites through the orbital movement of the earth.

Algol may, then, be regarded as the solitary luminous member of a multiple combination of opaque masses. The common centre of gravity, round which the pair hitherto known revolves in a period of about 131 years, lies by the present hypothesis at a distance from it just equal to that of Uranus from the sun. The path thus traced out is, we are further informed, sensibly circular, and its plane is inclined 20° to our line of vision. Obviously, however, during the whole time occupied in travelling over its remoter half, the light-minima of the star must be recorded somewhat later than if we saw them in the precise order of their actual occurrence; and this remoter half was swept over between the years 1804 and 1869, when the observed phases were always in arrear of calculation. Now, on the other hand, that the star is on the hither side of its orbit, the epochs of its eclipses are apparently anticipated, and will not coincide with their true times until the passage of the "ascending node" about 1934. The dimensions of Algol's orbit, together with its inclination, of course prescribe the amplitude of the oscillations by which its periodicity appears to be disturbed; and this "light-equation," as we may call it, proves to be 149 minutes. This should be the maximum extent, whether of acceleration or of retardation; but in point of fact, as we have seen, delay mounted up in 1843 to 165 minutes. Hence the theory cannot be said to represent the observations as satisfactorily as could be desired. The deviations, indeed, are large enough to suggest to Dr. Chandler further complications, the unravelment of which may challenge the utmost skill and patience of investigators. Meantime, a touchstone of the general truth of his hypothesis will soon be at hand; for it involves a cessation within the next ten or twelve years, and a subsequent reversal, of the shortening process at present affecting the star's period of luminous change; and the fulfilment of this prediction will serve as a hall-mark of its genuine quality. An additional test may be derived from spectrographic evidence. The velocity of Algol in the large orbit attributed to it is 2·7 miles a second; but of this, less than one half, or about one mile per second, is at present directed towards the earth. It constitutes, however, a goodly proportion of the 2·3 miles of continuous approach determined from the Potsdam plates; but which should in the course of a score of years, if the new theory be true, completely disappear, neutralized by the altered direction of the star's orbital motion. It remains, indeed, to be seen whether the whole of its supposed translatory speed may not really be of a circulatory character.

Dr. Chandler's theory does not rest wholly on the cyclical inequalities of Algol's light-changes. He alleges

* *Astronomical Journal*, Nos. 255, 256.
† *Ibid*, vol. vii., pp. 165-183.

also in its support periodical disturbances of proper motion, brought to view by a careful discussion of all the observations of the star, from 1753 to the present time, and indicating, in his opinion, a combination of elliptical travelling with a progressive advance. But the average proper motion of Algol is so very small—less than two seconds of arc a century—that variations or irregularities in it can at present be regarded only as an interesting possibility. They would give, if confirmed, 2·7″ for the longest diameter of the ellipse into which the wide orbit traced out by Algol round its unseen primary is projected upon the sky. And since this little span represents an actual expanse of 38 earth-to-sun distances, or "astronomical units," it implies a parallax for the star of 0·07″, corresponding to a distance of nearly 47 light years—a statement that is in many ways worth thinking about. Although claiming only qualified credence, it nevertheless conveys the upshot of assuredly the most promising attempt yet made to determine, by indirect means, the parallax of a star. In itself, too, it seems probable enough. Assuming its accuracy, we gain the information that Algol emits 63 times as much light as the sun, which, in its place, would show with little more than the brightness of a seventh magnitude star. The famous variable, moreover, according to Dr. Vogel, is just one million of miles in diameter, so that it presents only once and a third the solar radiating surface; yet it is, as a light-giver, 63 times more effective. The remarkable conclusion follows, that Algol is intrinsically 47 times more brilliant than the sun. The emissions from its photosphere are, per unit of area, 47 times more powerful. And should its parallax eventually—as seems not unlikely—prove to be smaller than 0·7″, this disparity will be still further enhanced.

By means, accordingly, of investigations of this nature, more fully and securely carried out, the question as to comparative stellar brilliancy may finally obtain a sufficiently satisfactory answer. It is a very important one. The process by which photospheric light is manufactured is still largely enigmatical; but the ideas commonly entertained about it are not easily compatible with the existence of considerable differences in the shining faculty of photospheric shells presumably identical in point of chemical composition. Reliable evidence of such differences has not hitherto been available. That light-power in stars bore no fixed proportion to mass was patent in numberless examples; but the density, consequently the dimensions of the emitting bodies remaining unknown, it could not be determined whether distension of substance, or innate strength of incandescence, was more concerned in producing a great sum-total of light relative to quantity of matter. The indications, however, now derived from Algol are overwhelmingly in favour of the latter alternative.

The primary member of its system, even if illuminated solely by the borrowed rays of its brilliant neighbour, may not, Dr. Chandler thinks, be out of reach of telescopic discovery. But his hopes, in this case, appear somewhat chimerical. It is not difficult to show that, under the circumstances supposed, a body of planetary constitution could not possibly be disclosed by any optical means at present available. Its position-angle relative to Algol is just now, we are told, 32°; while its distance from the same star is in the inverse ratio of its mass. This is considered by our author to be indeterminate; but it is not so, unless we reject Dr. Vogel's value for the combined mass of the close pair forming the variable. Assuming its approximate correctness, and that Algol and its immediate attendant accordingly contain two-thirds the solar quantity of matter, and admitting further that they revolve together, at a distance of nineteen astronomical units, in a period of 131 years, round their common centre of gravity with another body, it follows that the mass of that body is about equal to that of the sun, and that it circulates at twelve units of distance from the gravitational centre of the system. It should be found, this being so, if found at all, at an apparent interval of rather less than 2″ from Algol. The real gap of space separating them—the radius, that is to say, of Algol's relative orbit—would be measured by thirty-one radii of the earth's orbit; and the effectiveness for visual purposes of a still problematical body, shining by reflected light alone, can hence be estimated. If of the same density with Algol, it presents a disc of fivefold area, which, endowed with Jupiter's high reflective power, or an albedo of ·62, would possess a total lustre $\frac{1}{2,080,000}$ that of the original source of its radiance. This is equivalent to saying that it should be fainter by sixteen stellar magnitudes. Yet the suppositions introduced above are perhaps unduly favourable to conspicuousness. Evidently, however, an eighteenth magnitude star, in the close vicinity of one of the second, is far below discernment with any telescopic or photographic powers likely to be in use for a considerable time, if ever; so that visual confirmation of Dr. Chandler's theory can only be looked for if the unknown mass it has brought ideally into existence be in some degree self-luminous.

That theory, as he remarks, "has a much wider cosmological meaning than the mere explanation of the phenomena" of a single star. Most "eclipse-variables" exhibit irregularities of the same type with those of Algol, and which will doubtless prove amenable to a similar explanation. Moreover, an incalculable number of stars which, from our point of view, escape eclipse, unquestionably belong to systems organized on the same general plan. One such, indeed, is already known in α Virginis, a first-fruit of discovery in this particular branch; and Procyon, perhaps, is one of many others essentially resembling it, although inaccessible to spectrographic research, because revolving in planes nearly perpendicular to the line of sight. Thus, the intimate association of dark and bright bodies of the same order of mass would appear to be no exception in the universal order. And this scarcely allows us any longer to regard a sun-like condition as representing simply and solely a stage in the condensation of a primitively nebulous mass. Some further conditions are plainly needed to produce the brilliant and concentrated evolution of light characteristic of "suns."

Dr. Chandler concludes his valuable paper with an appeal for micrometrical measures of Algol stars, adapted to detect and determine possible systematic disturbances of their proper motions. Measures of the kind might, in his opinion, lead to highly significant results which would probably, in the case of Y Cygni, be reached with particular promptitude. For the cyclical fluctuation of this star is completed in about 600 single periods, or two and a half years, and has an amplitude of no less than four hours. Hence, the orbit described, on the light aberration theory, in that short period, must, even if its plane be coincident with the line of vision, approach that of Uranus in size, and the star's movement in it should accordingly be betrayed by vibrations exceeding many times in extent those due to its annual parallax. "If the research gave favourable results in this instance," our author continues, "it could then be extended to λ Tauri, which appears to be also a promising candidate." It is to be hoped that the suggestion will not remain unheeded. Owners of heliometers could hardly turn them to better account than by applying this simple criterion to an hypothesis which, if approved as true, opens yet one more road through the daily widening field of sidereal discovery.

THE GREAT EARTHQUAKE IN JAPAN OF 1891.

By the Rev. H. N. Hutchinson, B.A., F.G.S.

LAST year the Japanese empire was visited by one of those great catastrophes that every now and then cause widespread destruction to life and property. The country, as everyone knows, is a land of Earthquakes. It is calculated that at least five hundred shocks occur every twelve months. Japanese people talk of Earthquakes from day to day just as we, in England, discuss the weather, and hardly a day passes without some perceptible shaking.

Our present knowledge of Earthquake phenomena is largely due to the valuable labours of Prof. Milne in Japan. We propose, in the present paper, to give some slight account of the great Earthquake of last year as described in an interesting and beautifully illustrated book* by Professors Milne and Burton, containing twenty-nine large photographs, mostly taken by one of the authors for the Japanese University. These have been very artistically reproduced and printed by a mechanical process by Mr. K. Ogawa, so that they are as permanent as the paper they are printed on (which is itself a product of the Earthquake district, being manufactured only in Echizen). These illustrations will be found most helpful by anyone wishing to study the effects and phenomena of Earthquakes, for they give a far better idea than any amount of verbal description, and for the time being one almost seems to be transported to the district in question, and to be studying the effects on the spot.

The Nagoya-Gifu Plain, which has been so sadly devastated, is one of Japan's great gardens. It occupies the centre of the empire, and is in the prefectures of Aichi and Gifu. To give some idea of the destructiveness of this Earthquake it may be mentioned that the most severely shaken district, in many portions of which the destruction of buildings and engineering works was complete, extends over 4200 square miles. Brick buildings were affected over a still larger area, viz.: as far as Tokyo to the east, and Kobe to the west; but the disturbance made itself felt over an area of 92,000 square miles. The authors estimate that, if the Japanese islands presented a larger surface of land, the effects might have spread over an area of 400,000 square miles. They tell us that a disturbance occurred in the Mino mountains, and at once an area greater than that of the empire of Japan became a sea of waves, the movements being magnified on the surface of the soft alluvial plains. In Tokyo, more than 200 miles from the centre of the disturbance, the ground moved in long easy undulations, producing in some persons dizziness and nausea, the movements being not unlike what we might expect upon a raft rising and falling on an ocean swell. But near to this centre the waves were short and rapid; whole cities were overturned, the ground was rent, small "mud volcanoes" were created, and the strongest of engineering structures were ruined. The loss of life was fearful; about 10,000 persons were killed. 15,000 were wounded, 100,000 houses were levelled with the plain, whilst almost every building in the inner seismic region was shattered. From these effects it is concluded that the earth-movements in Mino at the time of the great Earthquake were at least equal to any movements recorded in the annals of seismology.

* "The Great Earthquake in Japan, 1891," by John Milne, F.R.S., Professor of Mining and Geology, Imperial University of Japan, and W. K. Burton, C.E., Professor of Sanitary Engineering, Imperial University of Japan. Published by Lane, Crawford & Co., Yokohama, Japan; Agent for this country, Edward Stanford, Cockspur Street, Charing Cross.

In speaking of the possible causes of Earthquakes, the authors point out that in the Nagoya-Gifu district there are neither volcanoes nor volcanic rocks; the plain is a bed of alluvium lying in a basin of palæozoic hills, and it was in these hills that the disturbance began. Hence it does not appear that this was one of those cases in which an Earthquake is connected with volcanic action, as many undoubtedly are. Rather it suggests a huge, internal, sudden jar, and possibly a slip or displacement, producing what is known to geologists as a "fault." In the general process of mountain formation, by which strata are compressed, contorted, and elevated above the level at which they were originally formed in lakes, seas, or estuaries, fractures must from time to time take place, when the internal strain becomes greater than they can bear. Geologists have sought the cause of such movements as these, by which mountain ranges are upheaved, in the secular cooling of the earth, whereby the outer layers cooling and contracting less rapidly than those down below, tend to be left unsupported, so that in settling down, they are thrown into folds, much in the same way as the skin of a dried apple is wrinkled. But it must be admitted that at present the subject of earth movements of all kinds —the slow movements producing elevation and depression of lands, or the sudden movements whereby the surface of the earth is shaken—is still involved in much obscurity.

Leaving these questions, let us turn our attention to the actual effects produced by the Great Earthquake in Japan last year. The greatest destruction has taken place along and near the river banks; the reason of this seems to be that, being unsupported on one side, the momentum of the shock has shot them forward much in the same way that the last of a series of railway waggons is shot forward when a locomotive bumps against the other end. Our illustration† shows the kind of destruction which has occurred along the banks of the Biwajima River for a distance of several miles. Innumerable longitudinal clefts occur, of all widths up to about two feet; also the inner half of the embankment has slid down towards the river to some extent, sometimes to a number of feet, measured vertically. At one place the embankment is entirely gone for a couple of hundred feet or so, and here a very strange thing has happened: a large bamboo grove and a few pines just at the back have been pushed sixty feet back, and yet the bamboos and trees remain upright! It will be seen that one thatched roof has fallen intact. The longitudinal cracks are well seen in the illustration.

The road from Nagoya to Gifu is a series of villages, or rather *was*, a nearly continuous street of more than twenty-five miles in length; *now*, except in a few cases, it is simply a narrow lane between two long heaps of *débris* that were once houses. The recent Earthquake teaches us that wooden houses, built on European models, have suffered less than ordinary Japanese dwellings, which have heavy roofs, no diagonal bracing, and light supports. No wonder that the heavy roof brings the whole structure down.

The disturbance seems to have been greatest in the famous Neo Valley, where the ground has been both elevated and depressed. The people say that the mountains themselves have been depressed, so that from certain points, hills, formerly invisible, can now be seen.

Earthquakes frequently cause landslips, and in this case we find that landslips were taking place for several days after. People who witnessed them were greatly impressed by the roaring noise and vibration. It is probable that the sounds are transmitted through the earth.

† Our Illustration is copied from the Photograph of Profs. Milne and Burton, by the kind permission of Messrs. Stanford.

THE BANKS OF THE BIWAJIMA RIVER AFTER THE GREAT EARTHQUAKE IN JAPAN, OCTOBER 27th, 1891.

From a Photograph by Professor JOHN MILNE and Professor W. K. BURTON, of the Imperial University of Japan.

The Bamboo Grove and Houses formerly on the embankment have slipped down into the bed of the Stream, the Trees still remaining upright. In the right hand foreground are some remarkable cracks, one of which might be mistaken for the body of a military man in a sitting posture.

There are some capital photographs illustrating the effects produced on the Nagoya Railway Bridge—a lattice girder-bridge. The broken cast iron piers lie on the dry shingly bed of the river, and the original line of the bridge has, in its central part, been deflected up stream. At one place a group of four or five houses has completely sunk into the earth ; only the roofs are to be seen, and on looking under them, instead of the confused heap of rubbish found under other houses, there is merely the surface of the ground, the same level inside as outside! The unfortunate persons who were in these houses at the time, lie buried, no one knows how deep.

It might seem at first sight as if it were quite impossible to mitigate the terrible effects of these disturbances, but experience teaches that there are ways in which much destruction to life and property may be avoided. Thus, it is known that on certain sites, buildings are not so much swayed and disturbed as on others only a short distance away. Thus the nature of the ground makes a good deal of difference, and buildings on soft ground, such as is generally found on plains, suffer more than those situated on hard rocky ground. River banks and the edges of cliffs are dangerous sites, because of the forward swing of the free face. The movements at the bottom of a pit, or even in a shallow railway cutting, are less than those upon the natural surface ; hence buildings rising from a pit, or with an area or basement, are less severely shaken. In building in a country much visited by Earthquakes, it is very important to construct with a view to resisting movements of a more or less horizontal nature—not merely up and down movements. Consequently, a heavy roof is a thing to be avoided, because by its own inertia it may break the support beneath, and come crashing down. In Italy arches are forbidden in Earthquake regions, for arches fall apart readily when acted upon by horizontal movements. A series of rules have been formulated for Japan, and it is much to be hoped that these rules will receive from builders the attention they deserve. The Nagoya-Gifu Plain is a region where Earthquakes are frequent. Violent disturbances took place in the northern part of Gifu in 1826, 1827, and 1859. But there is no doubt that in this district seismic activity is gradually becoming less; the occurrence of great disasters has been separated by longer and longer intervals. Many interesting problems in seismology have yet to be solved, but Prof. Milne's labours in Japan will at least form a valuable foundation for future workers in this field of research to build upon.

THE GREAT SUNSPOT AND ITS INFLUENCE.

By E. W. Maunder, F.R.A.S.,

Assistant superintending the Solar and Spectroscopic Departments at the Royal Observatory, Greenwich.

It would be a very difficult and toilsome task to obtain a satisfactory record of the perturbations of the magnetic needle, were it not for the assistance which photography has given us. But it is no longer necessary to have relays of observers reading the instruments at intervals of two minutes through every hour of the twenty-four ; by a very simple and effective arrangement the needles record their own positions, not merely at two minute intervals, but continuously. A light mirror is attached to the needle, and a gas lamp or other source of light is so arranged that a beam of light proceeding from it and falling on the mirror is reflected off to fall on a drum covered with sensitive paper. The drum is turned by clockwork and revolves once in the twenty-four hours. If then the magnet remains absolutely still, the spot of light falling on the paper will leave a blackened trace upon it which, under such circumstances, will form a straight line right round the drum. But if from any cause the magnet should twitch or turn to one side or the other, then the spot of light will be thrown up or down on the drum, and a zigzag will be introduced into the trace. As the light is shut off from the gas-jet by the clock at each hour there is then a slight interruption in the trace, and should a deflection be indicated it is only necessary to see between which hour-marks it lies, and to measure its distance between them to ascertain the exact minute at which the disturbance took place. In order that the hour-marks may be correctly identified, the light is cut off by hand at some specified time. In the copy of the trace for the twenty-four hours extending from noon of February 12th to noon of February 13th, given in the adjoining plate, will be seen that this break occurs at about a quarter to nine in the evening of February 12th.

One result of this automatic method of registration is that it becomes possible to ascertain whether or no a disturbance is truly simultaneous for the magnets at widely separated stations. This is a matter of first importance, for if it be clear that the magnets the world over are all disturbed at practically the same instant, we are precluded from finding the cause in anything restricted or local, and it becomes possible to accept an explanation which connects the disturbance with solar changes.

The accompanying plates are simple photographic copies, unmanipulated and unaltered, of the original automatic registers of two of the magnets at the Royal Observatory, Greenwich, for the period from noon on February 12th to noon on February 14th. The two magnets in question are those which record the "declination" and the "horizontal force" respectively—the declination, that is to say, the divergence of the needle from the true geographical north ; the "horizontal force" is a measure of the intensity of the magnetic action, but the total amount of that intensity is not observed directly ; it is observed in two directions at right angles to each other, the one horizontal, the other vertical, the record of the former only being given here.

From noon on February 12th, for seventeen hours, the trace shows that the magnets were almost quiescent. The bending down of the declination trace during the early afternoon of February 12th, its upward turn towards evening and through the night, until past midnight, are typical of the regular diurnal movement. At times of great stillness this gentle wave motion goes on day after day, unbroken even by the little breaks and ripples which the trace shows in this particular instance.

And now, before looking at the sudden and sharp disturbance which occurred at about half-past five in the morning of February 13th, let us briefly glance at the reasons already before us, for assigning the sun a dominant influence on terrestrial magnetism. First of all, there is this daily swing, westward in the morning and early afternoon, eastward in the evening and night, a swing which goes on perpetually day after day, which shows so clearly by its conformity to the length of the solar day, and by its being an action, not simultaneous over all the earth, but varying with the local time, that it is connected with the position of the sun relative to the place where the observations are being made. Next, there is a yearly, or rather, a seasonal variation, the amount of this daily change and the intensity of the magnetic force being least in the winter—the local winter—rising to a maximum a little before and again a little after midsummer. Here, again, is a second distinct, indisputable local effect, connected with the manner in which the given locality is presented towards the sun. The daily rotation of the

earth on its axis, the yearly revolution round the sun, are both distinctly recognised by the magnets, and, if necessary, we could determine the exact period of either by magnetic observations alone, if carried on for a sufficient length of time.

But the third relation is of a different order altogether. It is that to which I referred in the first part of this paper when I said that "when we take average results for successive years we find that this motion," *i.e.*, the diurnal range, "is greater in amplitude and force in strict proportion to the number and size of the spots upon the sun."

It is now more than forty years since the occurrence of a regular periodicity in the numbers and dimensions of Sunspots was established by the observations of Schwabe, a periodicity which, though now so familiar to us as a fact, still remains as much as ever a mystery as to its cause. Just before Schwabe published his great discovery Lamont had detected a similar periodicity in the oscillations of the magnetic needle, and directly Schwabe's Sunspot results were published General Sabine and several other observers of magnetic phenomena at once recognised that the two periods were not only similar but identical.

This identity did not consist in a mere equality of the average period, or in an occasional correspondence of minima and maxima. It was far more precise. The attempt was early made to explain the Sunspot cycle as due to the influence of Jupiter, the period of revolution of that planet nearly corresponding to the mean solar cycle. But the period of Jupiter is a constant, whilst the solar cycle is irregular in length, often varying considerably from its mean value. And the collation of earlier Sunspot records soon showed that, though a fair correspondence might be made out between the planet's position and the numbers of spots seen during two or three periods, yet if we went a little further back the two got hopelessly out of step. There was, therefore, no connection there.

But the correspondence with the magnetic cycle was precise. Was the interval between one minimum and the next shorter than usual with the Sunspots? Then it was so also for the magnetic diurnal range. Was the spot maximum delayed? So was the magnetic. The two cycles have never failed to correspond in their general features since both were first under regular observation.

So far is this the case that Dr. R. Wolf, one of the first of Sunspot observers, has worked out a very simple formula by which he is able to convert the yearly means of what he terms his "relative numbers" for Sunspots, so as to represent the yearly means for the diurnal range of magnetic declination as observed at Milan, and a similar formula would serve for other localities.

The two cycles correspond also in more than their general features or annual means.

The life of a Sunspot being almost always short, and spot groups varying very much as to size and being very irregularly distributed over the sun, it follows necessarily that even at a time of maximum there are very great fluctuations in the proportion of the sun's visible hemisphere which is given up to spots. So that the rise to maximum, or the fall to minimum, does not proceed smoothly, but in a succession of waves as it were. Of these minor oscillations, any one which has been fairly well marked and at all sustained is sure to have its counterpart in the activity of the magnets.

It would almost seem a necessary corollary of this last circumstance, that particular and individual Sunspots should be attended by corresponding magnetic disturbances. It certainly was the case with the great February spot, as we have but to glance at the accompanying plate to see. At about half-past five in the morning of February 13th, the trace both of the declination and of the horizontal force magnet suffered a sharp and sudden deflection, token of an instantaneous disturbance. This sharp and sudden twitch, which occurred, so far as we know, to the needles in all the magnetic observatories of the world and at practically the same actual (not local) time everywhere, is pre-eminently typical of the intense magnetic storms. An inspection of the trace shows that its later wanderings were far more considerable than this first one, but it is its instantaneousness which gives it its special character. In the afternoon of February 13th, the magnets were vibrating to such an extent that the two traces have become intermixed. Later, about midnight on February 13th, the magnets twitched so violently that for about an hour the trace went quite off the prepared paper. The motion in declination that was registered amounted to more than 1°. The horizontal force trace went off the sheet for an hour and a half, and in vertical force (not included in the plate) the disturbance was so great that the trace was lost in one direction, that of increasing force, for four and a half hours, and in the other for an hour and a half. A period of short, quick oscillations was then set up and continued for some hours, the disturbance dying away in the course of the evening of February 14th—a little later, that is to say, than the trace is shown on the plate.

So remarkable a magnetic storm, occurring just about the time when so great a spot was about attaining its fullest development, might well be taken as proving a special connection between the two, especially when the facts already alluded to with regard to the general correspondence between Sunspots and terrestrial magnetism are borne in mind. And the conclusion would be further supported by the occurrence of a fine aurora on that same night, February 13th, for the variation in number of auroræ has also been shown to take place precisely in the same cycle as the variation in number of Sunspots.

But the proverb tells us "One swallow does not make a summer," and neither can we take one coincidence as proving a real connection. Since the spot group of February, 1892, was the largest ever observed at Greenwich, let us look through the records for the group second in size, and see if that has any information to give us.

This second spot we find in that of November, 1882. This group was seen during three successive rotations of the sun. It formed in the visible hemisphere on October 20th, increasing in size on the succeeding days with marvellous rapidity. It passed out of sight at the west limb on October 28th, and was next seen at the east limb on November 12th. Its area on November 13th, when the whole of it was visible, amounted to 1690 millions of square miles, and this area increased day after day until November 18th, when it reached the central meridian and attained its greatest dimensions, having an area of 2860 millions of square miles. After this it began to break up and to diminish again. On November 23rd its area was 2030 millions of square miles. On November 25th it had reached the west limb. It was seen at the east limb again on December 10th, its area reduced now to 515 millions, and by December 21st it had completely faded out.

What was the record of the magnets at the time when this spot was at its greatest development? This is what the Astronomer-Royal chronicles: "A remarkable magnetic storm, preceded by several days of considerable magnetic disturbance, was observed here on November 17th. It commenced suddenly November 16th, 22h. 15m. G. M. T. with a great decrease in all the magnetic elements, the declination being diminished by more than 1°, the horizontal force by more than 1-100th part, and the

vertical force by nearly 1-100th part. From 4h. to 7h., and also from 11h. to 17h., the motions were large and violent, the range exceeding 2° for the declination, and 1-50th part for the horizontal and vertical force. Earth-current disturbances were also recorded, corresponding both in time and magnitude with the magnetic changes."

Mr. Whipple, Superintendent of the Kew Magnetic Observatory, supplements this information by some additional notes not less interesting. "The disturbance," he says, "commenced about 8.30 P.M. on the night of Saturday, the 11th inst." (That is to say at the precise time, so far as it is possible to fix it, when the great spot began to come into view at the east limb of the sun.) "Throughout the whole of Sunday, Monday, and Tuesday, the magnet continued slowly oscillating through arcs of about 20′ on either side of its normal position. On Wednesday and Thursday the vibrations were frequent, but very small, partaking rather of the nature of tremors.* About 10.30 A.M. on Friday † the storm became violent, and from that hour up to 5.30 A.M. of Saturday the oscillations of the magnet and the changes of force were incessant, and frequently enormous, the declination-needle ranging at times through almost 2°."

It is unnecessary to multiply quotations, but a similar record could have been gathered from any number of magnetic observatories, a record showing a restlessness of the magnets beginning just as the spot first came into view at the west limb, and rising to the intensest excitement just about the time that it came to the central meridian and reached its greatest development.

And not merely from observatories where the study of delicate magnetic phenomena is made a speciality were such occurrences reported. "All along the railway," writes an observer in Scotland, "the block instrument bells in the railway cabins were occasionally rung as if by the operator, and telegraphic communication was much interrupted, and in some cases even temporarily stopped." Another, in the south of England, reports: "At the local post office here all the longer lines were much affected during Friday and Saturday, sometimes to an extent interfering with ordinary messages." Nor was this effect only felt in the British Isles: the American and Continental newspapers recorded just as striking interferences with the usual action of the telegraphs.

Yet a further point. Just as the magnetic storm of last February was accompanied by a fine aurora, so was that of November, 1882. Indeed the 1882 record is the more remarkable. "Auroræ of varying brilliancy," Mr. J. Edmund Clark records, "were seen at York on the 12th, 13th, 14th, 15th, 17th and 18th (morning of 19th), November the 16th and evening of 18th being too cloudy for observation; the 17th giving an exhibition of exceptional brilliancy."

Records of the aurora of November 17th are plentiful enough, and no wonder, for it was the most remarkable exhibition of the kind seen for a generation, and an entire number of Knowledge might easily be filled with different reports of it. It will be amply sufficient, however, for my present purpose to quote the very brief account which the Astronomer-Royal gave of it:—"In the evening, as soon as it was dark, a brilliant aurora was seen, commencing with a bright glow of red light extending from the north and west beyond the zenith, interspersed with pale green phosphorescent light and streamers. At 6h. 4m. a very brilliant streak of greenish light, about 20° long, appeared in the east north-east, and rising slowly, passed nearly along a parallel of declination a little above the moon, disappearing at 6h. 5m. 59s. in the west, about two minutes after it was first seen. The whole aurora had faded away by about 7h., but it burst out again at 11h. 45m., when an auroral arch, with brilliant streamers reaching nearly to the zenith, was seen from north north-east to north-west. It faded away about 12h. 10m.

The "spindle-shaped beam," as the late Mr. Rand Capron called the remarkable appearance seen just after six o'clock, was of itself sufficient to distinguish the auroral display of November 17th, 1882, from ordinary auroræ, but though it naturally attracted the greatest attention, the other and more familiar details of the phenomenon rendered it the finest seen for many years. Thus Mr. J. G. Elger speaks of it as "certainly finer than that seen on October 25th, 1870."

This second instance of the simultaneous appearance of a great Sunspot, great magnetic disturbance, and fine aurora, very materially strengthens the argument for a true connection between the three orders of phenomena. Let us push the enquiry a step further.

The third and fourth spots as to area during the period covered by the Greenwich record were upon the sun at the same time. Group No. 726 of the Greenwich series was first seen on the extreme east limb on April 10th, when it was only of small size. It began on April 16th, however, to develop very fast, and the group which had an area of 258 millions of square miles on April 15th, on April 18th covered 1620 millions, and a second great outbreak had added more than 1000 millions more by April 21st. The group reached the west limb upon April 23rd.

Group No. 729 followed No. 726 at an interval of about 30° of solar longitude, and was about 10° further south in solar latitude. It was already a giant spot when first seen at the east limb on April 13th, its area being 1340 millions of square miles. It increased steadily up to April 16th when it covered 2430 millions of square miles, and after suffering a slight decrease it reached a second maximum of 2490 millions on April 21st.

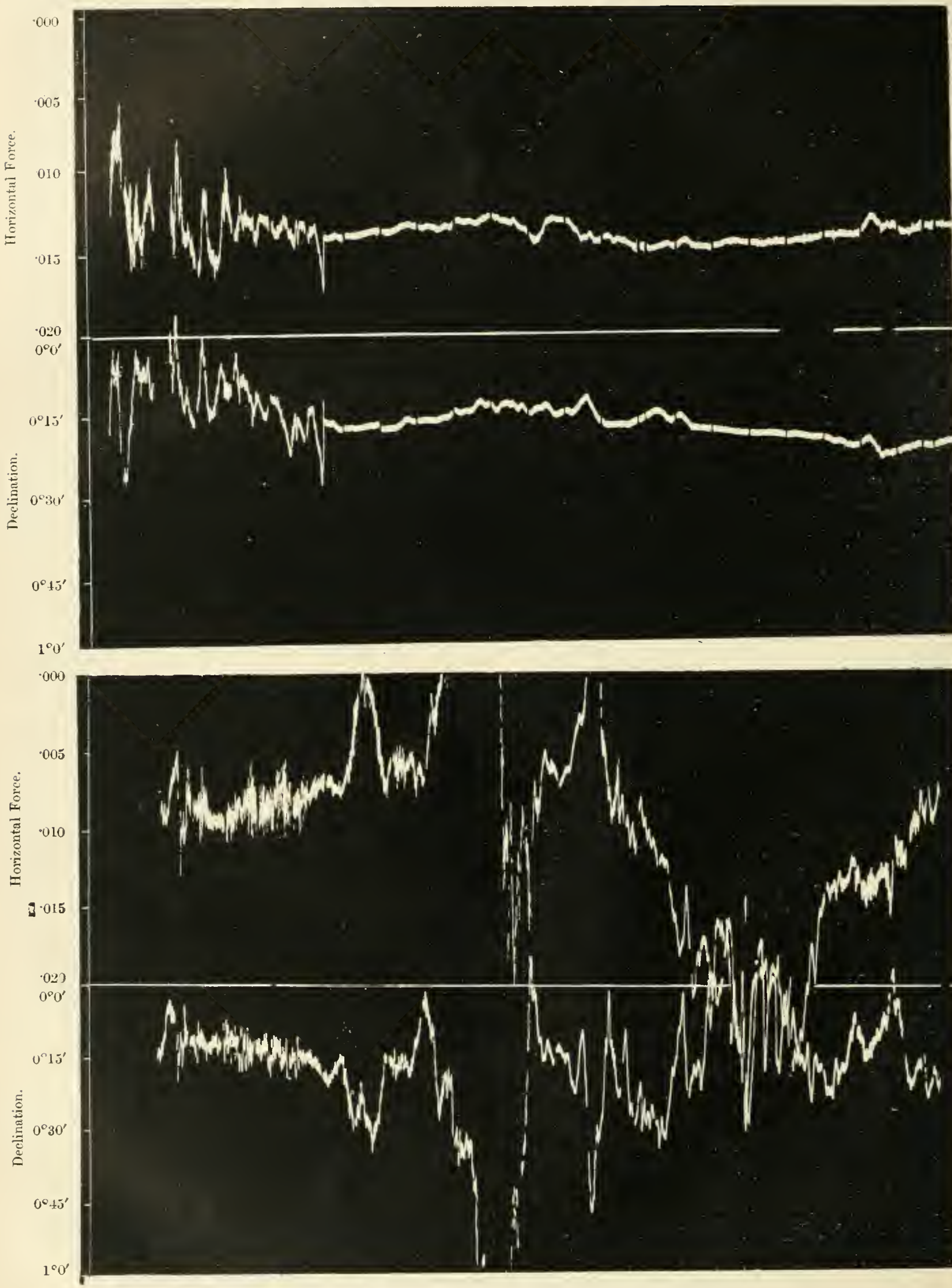
Had the magnets any record of disturbance to show? Yes, and a double one; the first on April 16th, when Group No. 726 was beginning its wonderful development and Group No. 729 had reached its first maximum; the second on April 20th, lasting till April 21st, simultaneous with the second development of Group No. 726, and the second maximum of Group No. 729. It is scarcely necessary to quote at length the official descriptions of this storm. I extract two sentences only from the account which the Astronomer-Royal gave: "The total spotted area was about double that of the greatest spotted area shown on any of the Greenwich photographs previous to this outburst"; and "since the great disturbances of 1872, February 4th, and those of October 4th of the same year" (both, I should remark, before July, 1873, when the Greenwich Sunspot record began), "no magnetic storm has been recorded equal to this."

In a period of nearly nineteen years, therefore, we have three magnetic storms which stand out pre-eminently above all others during that interval. In that same period we have three great Sunspot displays—counting the two groups of April, 1882, together—which stand out with equal distinctness far above all other similar displays. And we find that the three magnetic storms were simultaneous with the greatest development of the spots. Is there any escape from the conclusion that the two have a real and

* The Editor's expression, on page 70, "There is no brewing of a magnetic storm, it breaks out with full violence from its commencement," was not appropriate to the storm of November, 1882. It was "brewing" a good while before it burst. Similar minor disturbances were continued till the spot disappeared at the west limb.

[This seems to have been a series of magnetic storms, rather than one storm.—A. C. Ranyard.]

† Or in astronomical time, November 16th, 22h. 30m.



Traces of the Magnetic Storm of February 12th to 14th, 1892.

binding connection? it may be direct, it may be indirect and secondary only, but it must be real and effective.

Consider that the period in question is practically some 6800 days. A magnetic storm does not last many hours; a Sunspot soon declines from its greatest development, or soon passes away from the centre of the apparent disc. Suppose we take an outside limit, and give a period of two days to a giant spot to exercise its influence, or a magnetic storm to expend its violence; what are the probabilities against 3 out of 3400 of such periods of the one phenomenon agreeing with 3 out of 3400 of the other, if they are not related? If 3400 numbers were placed in one box, and 3400 more in a second, and one from each box were drawn at a time, what is the chance that the three highest numbers would be drawn from the one box, simultaneously with the three highest from the other, each to each, if the matter had not been pre-arranged? Indeed, we might legitimately call the coincidence of April, 1882, a double one, and ask the odds against the four highest numbers from each box being so drawn.

Between Sunspots and storms of the second magnitude it is more difficult to make a satisfactory comparison, because it is not so easy to frame a satisfactory definition as to what constitutes a secondary disturbance. Nevertheless, the following brief table of large Sunspots seen since the beginning of 1881 which were coincident with considerable disturbances may prove of interest. The spotted area is given in millions of square miles.

Date.		Spotted Area. Entire Sun.	Largest Group.
1881.	Jan. 31	1295	686
	Sept. 12	2089	917
1882.	Oct. 2	2480	1234
	,, 5	2065	1198
1883.	April 3	1545	607
	,, 19	2170	670
	June 30	3650	2210
	July 11	1887	1009
	,, 29	1425	1264
	Sept. 17*	2017	1263
	Oct. 16	4730	1733
	,, 20	1650	1369
	Nov. 1	2100	784
	,, 19	3682	1600
1884.	March 2	1510	609
	April 24	2348	1510
	,, 30	1746	897
1885.	Jan. 23	1687	592
	Feb. 5	1345	571
	,, 13	1569	480
	May 26*	1923	647
	June 24*	2348	1681
	July 18	1835	504
1891.	Nov. 22	1966	1371

Some of the above, those marked with an asterisk, may fairly be taken as confirming, though with less definiteness, the conclusion drawn from the correspondences between the greatest spots and the greatest storms. But with the others it is not so. Spots as important have been seen upon the sun, and the magnets have scarcely fluttered, and storms as distinct have occurred when there have been only few spots, and those but small, upon the visible disc of the sun. The table is important therefore, not as adding to the weight of the evidence in favour of the connection between Sunspots and magnetic disturbances, but as emphasizing a point which must not be forgotten. Though the diurnal and annual changes of terrestrial magnetism conclusively prove the solar influence upon it, though the connection between the general Sunspot cycle and the general magnetic cycle is clearly established, though even in minor irregularities the two curves closely correspond, and though unusually large Sunspots are answered by unusually violent magnetic storms, we cannot, as yet, proceed further and express the magnitude or character of the magnetic disturbances in terms of the spotted area of the sun, or of its principal groups at the time of observation. The conclusion to my own mind seems to be that though Sunspots are the particular solar phenomenon most easily observed, we must not therefore infer that their number and extent afford the truest indication of the changes in the solar activity which produce the perturbations we remark in our magnetic needles.

Letter.

[The Editor does not hold himself responsible for the opinions or statements of correspondents.]

DARK NEBULOUS MASSES.

To the Editor of KNOWLEDGE.

DEAR SIR,—I see that you take for granted the existence of dark nebulous masses. It appears the easiest hypothesis to account for the curious features shown in the Milky Way; and the evidence for it seems strong, but is it yet sufficient, or may there not be some other way of accounting for the dark features? The fact that the dark regions are so often closely surrounded by stars shows that there are peculiarities in the arrangement of the stars with respect to the dark areas. Is it not, therefore, possible that the dark regions may be simply spaces for some reason devoid of stars?

With reference to the tree-like dark object—(flower-like I should be more inclined to call it)—referred to on page 230 in your December number, there is one feature which is very striking to me that you show only partially, and that is a straight line extending in a slanting direction from the bottom of the stalk to the left. You draw the portion of the line to the right. The large photograph in the December number shows this feature very distinctly as three straight parallel lines of stars, of which the two outer ones are by far the plainest. The passages between them are very slender, the northern one so slender as to give one almost the impression of a scratch on the photograph. This shows the advantage of having copies from more than one negative to compare, as this feature is also visible in the photograph in your October number, only not quite so distinct. Other less distinct lines emanate from the structure, and give to my eye quite an appearance of radiation, which your drawing, Fig. 1, December number, only partly indicates. Nearly parallel with the straight line from the lower part of the flower-like structure but detached from it, there is another slender dark straight line, also visible on both your photographs. These dark lines remind one of the straight nebulous lines in Mr. Roberts' photograph of the Pleiades.

Yours faithfully,

Sunderland. T. W. BACKHOUSE.

[A close examination of the photographs shows that the density of the photographic action has been less within the dark areas referred to than over surrounding and neighbouring regions. This shows that the appearance of darkness is not an effect of contrast, and that there is either less nebulosity within the regions surrounded by stars, or that there is some opaque or semi-opaque matter within the dark regions which cuts down the light of the background. The dark areas are of various depths of blackness, as if some were due to masses of faint luminosity, while others were due to an opaque fog in space.—A. C. RANYARD.]

ANTS' COMPANIONS.—I.

By E. A. BUTLER.

IT has already been pointed out that an Ants' nest may contain more than one kind of insect, and we have shown how some Ants may be associated with others as guests, parasites, or auxiliaries. But when we have shown this, we have by no means come to the end of the matter: other insects, structurally unlike Ants, are also to be found in the nests intermixed with the true proprietors, and the alien population of this kind may, in a large community, be considerable. In the nests of the various European Ants there have been found nearly six hundred different kinds of insects other than Ants, and even this long list does not quite exhaust the catalogue of visitors; sundry species of invertebrate animals which are not insects, such as crustaceans, mites, &c., must be added to complete the total. Of the creatures hitherto registered as occurring in Ants' nests, however, by far the greater number are true insects, belonging to the order of beetles (Coleoptera). Some of these are, no doubt, accidental visitors and not true inhabitants: still, many are genuine lodgers in the nests, and some are found only in such situations. A very large proportion of the Ants'-nest beetles, again, are representatives of a single section of that great order, the division called Brachelytra, or Rove-Beetles. The scientific name of the group means "short wing covers," and refers to one of the most distinctive characteristics of the beetles (see Fig. 1): they carry their wings neatly packed away under two small squarish covers or elytra, which represent the fore wings, but which do not cover the abdomen as they do in most beetles, but lie over its extreme fore-part only, leaving all the rest exposed, giving thereby an earwig-like appearance to the larger members of the group, as was pointed out in the article on earwigs some months ago. Beetles of this kind are extremely numerous in the British Islands, where they usually act the part of scavengers, devouring carrion, dung, rotten fungi, and other animal and vegetable refuse. It is chiefly *small* species of these insects that inhabit Ants' nests, creatures scarcely larger, and sometimes much smaller, than the Ants themselves. Most people have probably noticed certain small species of Brachelytra, for an exceptionally warm and sunshiny day in early spring is sure to entice swarms of them from their hiding-places, when they will gaily disport themselves in our streets, running rapidly over the ground, or spreading their filmy wings and sailing through the air regardless of obstacles, often, therefore, proving themselves an annoyance by getting into the eyes of passers-by. The "fly in the eye" as often as not consists of these tiny insects. Such species would as a rule, however, not be the particular kinds that are found in Ants' nests, though they will serve very well to give a general idea of the form of these latter.

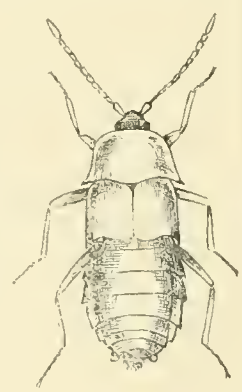


FIG. 1. — *Atemeles emarginatus*, a beetle living in Ants' nests, magnified 8 diameters.

The present season of the year is one of the best times to find these brachelytrous Ants' guests, and as the method of search for them is simple, a few words on the subject may perhaps incline some of our readers to look for them. The nests of the Wood Ant (*Formica rufa*) are some of the easiest and most profitable to commence operations upon. The mounds of these insects will now be just beginning to rise, the Ants waking to renewed vigour after their winter rest, and fortunately it will not be necessary to pull the nest to pieces, or otherwise greatly interfere with the labours of the workers in order to find the beetles: such an operation would be both disagreeable and needlessly destructive. All that has to be done is to select a few bricks or large flat stones, and place them on the nest sloping round its sides; a few broken bits of wood, such as fragments of dead branches, may usefully be added on the top of the nest, and a number of nests within easy distance of one another may be treated in a similar way. These pieces of brick, stone, and wood are intended to act as traps to induce the beetles to congregate on their under surfaces. A few days after the traps have been laid, the nests may be visited again, when it will most likely be found that the objects deposited are partially covered, by means of the labour of the Ants, with fragments similar to those that compose the rest of the mound. The traps may now be carefully but quickly lifted, one by one, and at once transferred in an inverted position to a cloth or a large sheet of paper, so that the insects upon them may be more easily seen, and have less chance of escape. Probably many Ants will be found clinging to the stones, but a quick eye will soon distinguish here and there amongst them strangers of some sort or other, which will probably turn out to be some of the beetles aforesaid. If it is desired to preserve the beetles, they may be gently removed and transferred to a bottle containing a few lumps of cyanide of potassium covered with pads of blotting paper; the poisonous vapour from this will soon kill them, and they may then be gummed on card for preservation and examination. As most of them are active in their movements, it will require a little dexterity to prevent their escape, and secure their safe transfer to the killing bottle. The fragments of stick at the top of the nest may be tapped or shaken over the cloth, or over a piece of white paper, when any insects that are in hiding will come tumbling out and may be secured. The stones and other traps may now be replaced, and the nests visited again at intervals during the spring or early summer, when no doubt other specimens will be obtained. It is best to visit the nests early in the day, before ten o'clock in the morning being, according to the late Mr. Janson, the best time to find the beetles. Of course, the method of procedure will be varied with the character of the nest; in the case of those Ants that do not throw up mounds of vegetable fragments, search must be made around the nest, and under any sheltering objects that may happen to lie about in the immediate neighbourhood. If the nest is under a stone, lifting this will often disclose some of the visitors, and again, the "runs" of the Ants may be profitably inspected.

The terms on which the beetles are associated with the Ants do not appear to be the same in all cases, and indeed in many instances it cannot be said to be satisfactorily determined what the terms are. Speaking generally, it is obvious that, to say the least, the presence of the beetles must in all cases be tolerated; for such is the energy and courage of Ants that they are not likely tamely to submit to the intrusion of interlopers which are decidedly distasteful; such would be at once attacked and killed, the jaws of the workers being quite capable of thus executing the capital sentence. Mere toleration and indifference, however, by no means represents the attitude of the Ants in all cases; in some instances there seems to be a certain amount of distrust and suspicion, but in others quite the reverse. Some are on terms of the closest intimacy with their hosts, by whom they are treated carefully and tenderly, and their presence is evidently valued. Speaking

at present only of the Brachelytra, and remembering that the general function of these insects in Nature is that of scavengers, one sees that it is not at all unlikely that such a function is discharged in the Ants' nests by at least some of the species found therein. The need of some arrangement analogous to sewage works, to dispose of the considerable amount of excrement and other refuse matters that must tend to accumulate in large communities of Ants, is obvious, and it may well be that a share at least of this work falls to the lot of the Ants' guests. Dr. Michael has recently shown that in the nests of certain Ants which he found in Corsica a kind of mite occurs, to which the Ants seem greatly attached; and a reason for this has been suggested by him as being found in the fact that the mites feed upon the bodies of dead Ants, and thus help the members of the community to a solution of the vexed question of the disposal of the dead, which cannot but be a matter of perplexity to all societies and great aggregations of living beings, increasing in difficulty in proportion to their size. In most cases the dead seem to be carried out of the nest, and the Rev. Farren White records a curious instance in which some little paper trays, which he had put upon one of the nests he kept for observation, were used as cemeteries, the Ants dropping the dead bodies of their companions into them one after the other. From the method in which the nest was in this case preserved for observation, viz., in a glass bell-jar, the bottom of which was wholly occupied by the nest, it would seem that this was the only method available by the Ants of putting the corpses into any spot where they could really be considered to be removed from the nest, and the Ants certainly deserve credit for the ingenuity of the device by which they overcame the difficulty presented by the glass walls of their prison.

No doubt in some instances the Ants and their guests are found together merely because they happen to prefer similar situations to live in, so that in such cases no particular significance can be attached to the association. Thus, if both delight to live under the bark of trees, or amongst the roots of grass, here is a bond of connection which will lead to their being found together unless either party objects; still, even in such cases, as the Ants, being always the more numerous of the two, could if they liked rid themselves of their companions, it is plain that the association is not distasteful to them. The mere accumulation of piles of vegetable fragments, such as those erected by the mound-builders, would tend to induce beetles to enter, as they often delight to lie concealed amongst vegetable rubbish, and it is certain that if any such mound were deserted by the Ants, it would very soon be tenanted by all sorts of different insects, as well as woodlice, centipedes, millipedes, &c. That such indefinite tenantry does not occur while the place is occupied by the Ants must therefore be attributed to their own efforts. Again, when fragments are brought in for addition to the mound, it is quite possible that they may sometimes contain upon their surface or in their interior either small insects or the eggs of such, so that we have here another possible means of the introduction of insects into the nests. On the whole, therefore, it would seem to be, not so much a matter of surprise that insects other than Ants are to be found in the nests of the mound-builders, but rather that their number is not even greater than it is. Of course, these remarks do not equally apply to those which do not throw up mounds; but then these do not, as a rule, yield so great an alien population.

We may now proceed to consider somewhat more in detail some of the most remarkable of these companions of Ants. Of the Brachelytra, one of the most curious is *Atemeles emarginatus* (Fig. 1). It is a reddish-brown insect scarcely more than a sixth of an inch in length, but broad out of proportion. It has a broad shield-like thorax, ending in thin leaf-like edges at the sides, and in sharp-pointed projections at the hinder angles. It has the habit of curling up the tip of its abdomen as it runs about, and this of course makes it look smaller than it really is. It can also roll itself up almost into a ball. It is so peculiarly shaped that it cannot easily be confounded with any other British insect. It is found in the nests of several species of Ants, especially the red stinging Ants (*Myrmica*), the blackish-brown *Formica fusca*, and the shining black *F. fuliginosa*. A good many of the beetles may often be obtained from one nest, though always the Ants far outnumber them. Wasmann records having found as many as one hundred and fifteen specimens in one nest of *Myrmica scabrinodis*. This is one of the insects that are very tenderly cared for by the Ants. It lives with them on terms of the closest intimacy, being often carried about by them from place to place. But more than that, the beetles are, at least to some extent, actually supported by the Ants, being fed by them from their own mouths, just as they would feed their young. If the beetle wants food, it approaches an Ant, and planting itself in front, with its tail cocked up, begins to stroke the Ant with its antennæ; the Ant soon responds and passes food into its mouth, continuing the operation perhaps for about half a minute, when it goes off about its business. Sometimes indeed the feeding takes longer, and some two or three minutes may be occupied in the process. It must not be imagined from this, however, that *Atemeles* has lost the power of feeding itself; this does not appear to be the case, although, according to Wasmann, the beetles flourish better when fed by the Ants. The same observer has mentioned that the Ants lick with apparent gratification certain tufts of yellow hairs which are found on the beetle's body, as though they yielded some secretion which was pleasant to the taste. He also mentions that the beetles have the power of exhaling an agreeable aromatic odour from a gland in the abdomen, similar to, but stronger than the scent of *Myrmica*, which arises from the head. The beetle enjoys a good deal of freedom of movement, notwithstanding the care of the Ants, and during the summer months it is not to be found in the nests. But if the beetles at other times manifest a tendency to wander too far, they are picked up by their guardians and carried back again. We have here, then, the case of a beetle which is closely associated with the life of the Ants, and is carefully tended, guarded, and fed by them, and yet which has so far retained its faculties and powers almost if not entirely unimpaired, and does not seem to have become degraded by its dependence. That such, however, is not always the case will be evident from other instances, which want of space compels us to defer till next month.

(To be continued.)

HOT SPRINGS.

By Prof. J. Logan Lobley, F.G.S.

THERMAL springs vary so greatly in temperature that to a large number the word "hot" is not applicable, and yet these cannot be excluded from consideration in connection with the subject of Hot Springs, since they are essentially of the same class of phenomena.

All springs of water the temperature of which is higher than that of the rocks from which they issue may be

regarded as thermal springs, since they have derived their exceptional heat from subterranean, and therefore extra-meteoric, sources.

The thermal springs of non-volcanic countries, although frequently but slightly heated, are in some respects the most noteworthy, for the cause of their heat, not being obvious, an attractive and interesting enquiry is suggested. Such are the thermal springs of England and Germany.

In England five have long been known. The most famous and the hottest is that of Bath, which has a temperature of 120° Fahrenheit, and then follow those of Buxton at 83°, Clifton at 76°, Matlock at 68°, and Ashby-de-la-Zouch at 62°. Very similar in many respects to the English springs are those of Germany, which have a world-wide fame, and are the resort of an annually-increasing number of both health and pleasure seekers. The range of temperature of these waters is considerably greater than that of our home springs, the Sprudel Quelle of Carlsbad having a temperature of 167° Fahrenheit. With the exception of the springs of Gastein, they are all in three districts—Nassau, the Schwarzwald or Black Forest (parts of Baden and Wurtemberg), and Bohemia. Their temperatures are as follows:—

Carlsbad, Bohemia (the Sprudel Quelle)	167° F.
Wiesbaden, Nassau (the Kochbrunnen)	158° ,,
Baden, Baden (the Ursprung)	153° ,,
Toeplitz, Bohemia (the Stadtbad)	122° ,,
Gastein, Austria (the Spital Quelle)	118° ,,
Ems, Nassau (the Kesselbrunnen)	118° ,,
Wildbad, Wurtemberg (the Furstenbad)	98° ,,
Schlangenbad, Nassau (the Altbad)	86° ,,
Liebenzell, Wurtemberg	76° ,,
Soden, Nassau	75° ,,
Cannstadt, Wurtemberg	66° ,,

Hotter, however, than any of the above-named waters is that of Abano, in North Italy, about five miles from Padua, which has a temperature of 187° Fahrenheit. It is true this place is at a seat of former volcanic activity, the Euganian Hills, but the period of that activity was as far back as Eocene times.

One of the most remarkable examples of Hot Springs in non-volcanic regions is that of Hammam-Meskhoutim, near Constantine, in Algeria, where the water, with a temperature of 203°, deposits calcareous travertine, and so forms piles of basins rising to about 30 feet. Another great deposit of travertine from such Hot Springs is at Hierapolis, in Asia Minor, where it forms white terraces on the side of the hill, on which in ancient times the water, at about 100° Fahrenheit, issued.

All these thermal springs, and many others in various parts of the world, agree in issuing from the rocks of regions of a markedly non-volcanic character, as those of England, or of areas where volcanic activity is apparently quite extinct. They all, therefore, require some hypothesis to account for their high temperature other than proximity to volcanic action.

The theory founded on the ascertained increase of heat with descent from the surface would put the source of the Bath water at about 4000 feet depth. A much less depth would be required by ascending hot fumes or gases meeting with water, and so heating and impregnating it and forcing it to the surface. The "physio-chemical hypothesis" that accounts for the production of volcanic heat at moderate depths by subterranean chemical action, when the conditions are favourable, appears to meet the case of the Bath waters, and that of other thermal springs in non-volcanic regions.*

* Reports of the British Association, Bath Meeting, 1888, p. 670.

One feature they have in common with all Hot Springs, wherever they may be found and of whatever temperature they may be, and this is that they are all more or less "Mineral Springs," and it is to this fact, together with their high temperature, that they owe their medicinal or curative reputation. Water is at ordinary temperatures a solvent of many substances, but at high temperatures of a greater number, and, too, of a greater quantity of those soluble in it when cold; and boiling water is a solvent to some extent of that usually most refractory substance, silica. Hence it is that boiling springs generally deposit from their cooling overflow water the solid silicious material, silicious sinter. It has been estimated that the matter held in solution by the Bath water, which has been derived from the rocks through which it has passed, would, if solidified, form annually a column having a base of nine square feet and a height of 140 feet. This matter consists chiefly of the sulphates of lime and soda and the chlorides of sodium and magnesium. A spring at Clermont, in France, has deposited a great mass of calcareous rock, forming a natural bridge of travertine, and other examples have been previously mentioned.

The thermal springs of volcanic regions which, from the evident cause of their heat, may be called volcanic thermal springs, are very conspicuously different in character, some being tranquil flows of heated water, some being in a state of ebullition more or less active, and others, the Geysers, being characterized by periodic ejections or eruptions of boiling water followed by violent explosions. There are, too, many thermal springs where there is at present no volcanic action, but yet these areas are so volcanic in character, and have been the seat of volcanic activity so late as post-Pliocene times, that the heating of the waters issuing from their rocks can only be ascribed to lingering volcanic fires.

Of these regions the old province of Auvergne, in France, is the most important in Europe. It has many thermal springs, of which those at Vichy, Vic en Carladez, Mont Dore les Bains, La Bourboule, and St. Nectaire are best known. And in America Hot Springs are conspicuous in such areas, in Utah, near Salt Lake City, where is Hot Springs Lake, and in Colorado, where are the important Pagazo Springs, and especially so in Wyoming.

The oldest known and longest used Hot Springs are those of the volcanic region of Southern Italy, where, near Naples, these *thermæ*, so greatly used by the patricians of old Rome, still give forth waters at the Baths of Nero, near the Lucrine Lake, of a temperature of 182° Fahrenheit, and at Pisciarelli, near the not yet quite extinct Salfatara, of 180° Fahrenheit, sufficiently hot, Dr. Daubeny found, to boil an egg in a few minutes; and across the blue waters of the Bay of Naples, on the volcanic island of Ischia, under the shadow of the slumbering volcano Epomeo, there are the now much used hot baths of Gurgitello. Some of the thermal springs of the remarkable volcanic district of the Phlegræan Fields are, however, of but moderate temperatures. At Bagnoli, near Pozzuoli, are two springs of 104° F., and the tepid waters of Baiæ made that place great and rich in ancient Roman times.

But it is that northern, indeed Arctic land, Iceland, of which most of us think when Hot Springs are named, for of these phenomena the geysers of Iceland received until recently the most prominent notice. There are other Hot Springs in Iceland, but geysers, or intermittent eruptive Hot Springs, are very numerous, both in the Hecla district and in other parts of the island.

The group best known consists of about a hundred, but so near together that they are all within an area of about two square miles. This place is about thirty miles from the active volcano Hecla, in the south-western part of Iceland. Two of the geysers—which are so named from the Icelandic word *geysa*, to rage — are conspicuously greater and more important than the others, and the larger of the two, called Great Geyser, has been carefully observed and described, first by Mackenzie, and afterwards by Bunsen, Descloiseaux, and others.

This intermittent natural fountain of boiling water has been found to have a shaft or tube descending to about 78 feet below the surface, with a diameter varying from 6 to 8 feet, which at the top expands into a funnel-shaped mouth or basin, 56 feet by 46 feet, that is really the crater of a mound sloping on all sides, from its margin to the level of the adjoining land. With intervals of about six hours between, a column of hot or boiling water is ejected high into the air, 100 to 200 feet. After this, which continues for five or six minutes, an explosion of steam occurs, the tube is emptied, and there is no more ejection of hot water for another period of six hours. During this time the tube gradually refills, when there is another ejection followed by a violent explosion of steam as before. Thus, throughout the year, amid the ice and snows of Iceland, this extraordinary periodical ejection of boiling water and steam goes on. The throat, the crater, and all around this aqueous volcano is coated with a deposit of silicious sinter, and it is an accumulation of this hard white material that has formed the mound and the sides of the basin from the centre of which the tube descends.

Although the great increase in recent years of our knowledge of the physical features of the earth's surface has made us acquainted with many other and greater eruptive boiling springs, yet the geysers of Iceland retain their place as types, and the hypotheses to explain geyser action are founded on the phenomena they present. Of these one of the first was that of Sir J. Herschel, who supposed a rock cavity holding water with a conduit from it to the surface passing through a stratum of very hot rock which would so greatly heat the water in passing as to cause the eruptive action. It was Bunsen, however, who was the author of the now generally accepted hypothesis — that the water as it collects from the surrounding rocks, in the lower part of the shaft, is heated by a hot zone to much over boiling point, as it was actually found by Bunsen to be 260° Fahrenheit within the tube, but that the pressure of the column of water above prevented the formation of steam. This goes on until the force generated overcomes the pressure, when the column of water above is ejected, and the lower water being relieved of the restraining pressure is suddenly converted into steam, and so produces the final explosion and emptying of the shaft. This action has been imitated by an arrangement devised by Professor Tyndall, who also illustrated the bringing about of premature eruptions of the geyser called the Strokkr by obstructing its tube with clods of earth.

A much grander exhibition of thermo-hydraulic action was displayed on the opposite side of the globe, when Von Hochstetter, in 1859, explored the volcanic regions of the North Island of New Zealand. From one point could be seen on both sides of the valley of the River Waikato 76 clouds of steam arising from hot cascades, falling into the river from white basins, some with rising and falling fountains, some pausing, some playing simultaneously, and so forming a wonderful system of grand natural water-works. The steaming cascades fell over white, red, and yellow terraces, while periodical eruptions occurred at points between them.

Still greater wonders were found to the north-west of the valley of the Waikato, where was the now famous lake of Roto Mahana, a lake of warm water, amidst boiling springs that were continually pouring into it immense volumes of hot water over marble-like terraces, on the hill slopes around. On a small island in the lake, potatoes and meat could be cooked by the steam given out whenever a little hole was dug in the ground. Te Tarata, "the Tattooed Rock," rose on the east side of the lake to a height of 80 feet in snow-white terraces, and on its summit was an immense basin filled to the brim with water boiling, but clear and of a brilliant blue tint. The white terraces over which the water fell in cascades to the lake, formed by the deposition of silica by the cooling water, had on each level basins, with projecting semicircular margins, and holding bright blue water, from which stalactites of pure silica depended, and added a light and fairy-like beauty to this marvellous piece of natural architecture. Other boiling springs, with descending terraces, were on this side of the lake, and they were confronted on the west side by Otukapuarangi displaying its pink terraces, which, with the blue waters surrounded by red, white, and yellow walls of rock, added additional colour to the wonderful scene.

It has been necessary to write of these marvels of the Roto Mahana lake in the past tense, as they are things of the past, for in June, 1886, a great volcanic eruption completely destroyed the terraces, the springs, and even the lake itself. Had, therefore, this part of New Zealand not then been explored, mankind would not have known of these magnificently beautiful wonders of Nature in the Britain of the Southern Seas.

To find a still more extensive display of Hot Springs we must return to the northern hemisphere, and visit that remarkable area in Wyoming, the Yellowstone National Park, in which are congregated so many natural wonders, and which has recently been described in these pages.

The thermo-aquatic phenomena here seen are on a much grander scale than anywhere else, but it is impossible now to do more than very briefly state the general character of these marvels. In addition to other parts of this region, in which there are numerous Hot Springs, both tranquil and eruptive, there is an area occupied by an unexampled development of the one class, and a second in which are concentrated extraordinary groups or collections of the other class. The former of these areas is that of the "Mammoth White Mountain Hot Springs." These in their character and surroundings resemble those of the destroyed White and Pink Terraces of New Zealand, but instead of silicious they deposit calcareous matter, and they are on a larger scale. The terraces are as brilliantly white, the water is as clear and blue, and the other colours are as varied and vivid as were those around the Roto Mahana lake. Stalactites, coral-like forms, and exquisite bead-work ornament the terraced basins and the platforms that cover the entire face of the mountain, rising from the banks of Gardiner's River at 5845 feet above the level of the sea to 6522 feet. An analysis of the travertine of the terraces formed by these springs, given by Dr. Hayden, is as follows :—

Water and volatile matters ...	32·10	per cent.
Lime	57·70	,,
Silica	3·32	,,
Ferric Oxide	3·62	,,
Alumina	3·31	,,
Soda and magnesia	traces	
	105·05	,,

The other most noteworthy Hot Springs area of the Yellowstone Park is called the "Geyser Basins of the Fire Hole River," consisting of the Lower Basin and the Upper Basin, altogether about 75 square miles.

The Lower Basin contains thousands of springs of various temperatures, including some gigantic geysers of which the "Fountain" and the "Architectural" geysers are the best known. There are also "Mud Puffs," or centres of boiling mud. The Upper Basin has geysers of still greater importance and becoming famous under the names of "Old Faithful," "The Giant," "The Giantess," "The Grand," "The Castle," "The Beehive," &c. All these, like the Geysers of Iceland, deposit silicious sinter or geyserite, of which Dr. Hayden's analysis is:—

Water	13·42	per cent.
Silica	76·80	,,
Alumina ...	9·46	,,
Lime	1·80	,,
Iron, magnesia, and soda ..	traces	
	101·06	,,

Although the Yellowstone Valley has long ago ceased to be the scene of volcanic activity, great subterranean heat still lingers beneath its mountains, cañons and lakes, and now presents to the admiration of mankind phenomena as beautiful as they are wonderful.

THE FACE OF THE SKY FOR MAY.

By Herbert Sadler, F.R.A.S.

Small groups of spots and faculæ continue to appear on the solar surface. The following are conveniently observable times of the minima of some Algol-type variables (*cf.* "Face of the Sky" for April). S Cancri.—May 11th, 9h. 32m. p.m.; May 30th, 8h. 48m. p.m. δ Libræ.—May 4th, 8h. 28m. p.m.; May 11th, 8h. 3m. p.m. U Coronæ.—May 13th, 10h. 49m. p.m.; May 20th, 8h. 31m. p.m.

Mercury is technically a morning star throughout May, but is practically invisible, as on the first day of the month he only rises 24 minutes before the Sun, and on the last day of May he rises three-quarters of an hour before the Sun. Under these circumstances an ephemeris of the planet would be useless. He is at his greatest western elongation ($25\frac{1}{2}°$) on the 17th.

Venus is a superb object in the evening sky, and is visible to the naked eye at noonday when her position is accurately known. She sets on the 1st at 11h. 49m. p.m., with a northern declination of 26° 49′, and an apparent diameter of 23″, just one half of the disc being illuminated. On the 15th she sets at 11h. 50m. p.m., with a northern declination of 26° 30′, and an apparent diameter of 28″, $\frac{41}{100}$ths of the disc being illuminated. On the 30th she sets at 11h. 21m. p.m., with a northern declination of 24° 35′, and an apparent diameter of $35\frac{1}{2}''$, just three-tenths of the disc being illuminated. During the month she pursues a direct path through Gemini, without approaching any conspicuous star very closely. At about 10h. 20m. p.m. on the 3rd a 9th magnitude star will be just north of the planet, and at 10h. 30m. p.m. on the 13th a $9\frac{1}{2}$ magnitude star will be very closely north of Venus, while at 10h. 10m. p.m. on the 21st a $9\frac{1}{4}$ magnitude star will be immediately south of the planet.

Mars does not rise till after midnight on the last day of the month, and Jupiter is, for the observer's purposes, invisible.

Saturn is still well situated for observation. He rises on the 1st at 2h. 34m. p.m., with a northern declination of 4° 43′, and an apparent equatorial diameter of 18·5″ (the major axis of the ring system being 42·8″ in diameter, and the minor 0·4″). On the 31st he rises at 0h. 32m. p.m., with a northern declination of 4° 48′, and an apparent equatorial diameter of $17\frac{3}{4}''$ (the major axis of the ring system being 40·8″ in diameter, and the minor 0·3″). The ring system is therefore invisible in small telescopes. The following phenomena of the satellites may be observed (the times are given to the nearest quarter of an hour). May 1st, $1\frac{1}{4}$h. a.m., Tethys, eclipse reappearance; May 2nd, $10\frac{1}{2}$h. p.m., Tethys, eclipse reappearance; May 4th, 8 p.m., Tethys, eclipse reappearance; May 9th, $8\frac{3}{4}$h. p.m., Dione, eclipse reappearance; May 18th, 1h. a.m., Tethys, eclipse reappearance; $1\frac{3}{4}$h. a.m., Dione, eclipse reappearance; May 19th, $10\frac{3}{4}$h. p.m., Tethys, eclipse reappearance. Iapetus is at his greatest western elongation on the morning of May 27th. On the 9th at about 11h. p.m. a $9\frac{1}{2}$ magnitude star will be about $\frac{3}{4}'$ south of the planet. During May Saturn describes a very short retrograde path through part of Virgo, without approaching any naked-eye star.

Uranus is well situated for observation, rising on the 1st at 6h. 25m. p.m., with a southern declination of 12° 17′, and an apparent diameter of 3·8″. On the 31st he rises at 4h. 20m. p.m., with a southern declination of 11° 54′. During the month he describes a retrograde path to the N.W. of λ Virginis. A map of the path of Uranus is given in the *English Mechanic* for February 12th. Neptune is in conjunction with the Sun on the 29th.

There are no very well marked showers of shooting stars in May.

The Moon enters her first quarter at 7h. 12m. p.m. on the 3rd; is full at 10h. 59m. p.m. on the 11th; enters her last quarter at 2h. 53m. p.m. on the 19th; and is new at 5h. 49m. a.m. on the 26th. She is in apogee at 5·3h. a.m. on the 9th (distance from the earth 252,310 miles): and in perigee at 4·6h. p.m. on the 24th (distance from the earth 224,215 miles). Her greatest western librations occur at 0h. 54m. p.m. on the 2nd, and at 5h. 30m. p.m. on the 30th; and her greatest eastern at 5h. 35m. a.m. on the 18th. There will be an eclipse of the Moon on May 11th, the first contact with the penumbra taking place at 7h. 55·9m. p.m.; the first contact with the shadow (at an angle of 82° from the most northern portion of the Moon's limb towards the east, for *direct* image); the last contact with the shadow at 0h. 36·6m. a.m. on the 12th (at an angle of 41° from the most northern portion of the Moon's limb towards the west, for *direct* image); and the last contact with the penumbra at 1h. 50·9m. a.m. on the 12th. The middle of the eclipse occurs at 10h. 53·4m. p.m. on the 11th, $\frac{953}{1000}$ths of the lunar disc being obscured.

Chess Column.

By C. D. Locock, B.A.Oxon.

All communications for this column should be addressed to the "Chess Editor, *Knowledge Office*," and posted before the 10th of each month.

Solution of Problem in April number.—1. Q to B7, and mates next move.

Correct Solutions received from H. S. Brandreth, C. T. Blanshard, A. H. C. Hamilton, "T. Wells."

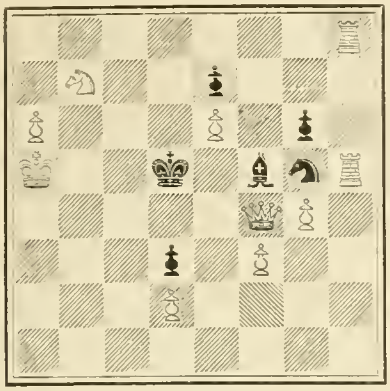
"*T. Wells.*"—1. BR6 will not solve the March problem. The move fails against one defence only, viz.: 1. . . . K to B5.

C. T. Blanshard.—Thanks for the problem; it is printed below.

PROBLEM.

By C. T. BLANSHARD.

BLACK.



WHITE.

White to play, and mate in two moves.

CHESS INTELLIGENCE.

The result of the Amateur Championship Tournament of the British Chess Association was as follows:—

1. E. Jones-Bateman	$9\frac{1}{2}$
2. H. W. Trenchard	7
3. E. O. Jones...	$6\frac{1}{2}$

Mr. Jones took the third prize after a tie with Mr. Herbert Jacobs, who is fourth. The other competitors were Dr. Smith, Messrs. Ward-Higgs and Gibbons, all of whom met with some success in the recent City of London Club Tournament.

It will be seen that Mr. Jones-Bateman won with a good deal to spare, Mr. Trenchard being rather a bad second. This is the first occasion on which the winner has not been a member of the British Chess Club. Mr. Jones-Bateman is the present holder of the Löwenthal Cup, which carries with it the championship of the St. George's Chess Club.

THE CHAMPIONSHIP TOURNAMENT.

At the conclusion of the National Masters' Tournament Mr. G. Newnes, M.P., the president of the British Chess Club, offered prizes amounting to £50 for a two-game tournament between the five leading London players. The masters selected were Messrs. Bird, Blackburne, Gunsberg, Lasker, and Mason. As in the previous tournament, Mr. Lasker again came out first, and, but for his willingness to accept draws with Mr. Mason, could no doubt have increased the distance between himself and Mr. Blackburne, whom he defeated by very fine play in both games. Mr. Blackburne, moreover, was extremely fortunate in winning his second game with Mr. Bird, a game which he played only to draw by perpetual check. He was also a little lucky in winning two games from Mr. Mason, both of which looked like certain draws. On the other hand, his first game with Mr. Bird was a fine specimen of counter-attack. Mr. Mason played as well as he usually does, while the other two competitors were apparently out of form. The following was the score:—

1. E. Lasker	$6\frac{1}{2}$
2. J. H. Blackburne	6
3. James Mason	4
4. I. Gunsberg	$2\frac{1}{2}$
5. H. E. Bird ...	1

The prizes were divided on the Sonneborn-Berger system, *i.e.*, according to the *value* of the games won by each player, estimated by the scores of the players from whom they were won. On this system Mr. Lasker, who did not lose a single game, came out easily first, and his performance confirms the estimate of his capacity given in this column last month.

THE UNIVERSITIES WEEK.

The boat-race week was, as usual, crowded with matches in which the Oxford and Cambridge teams were engaged. The first contest, between past members of the two clubs, was decided on the Tuesday at the St. George's Chess Club. The old Oxonians, who missed the services of Messrs. W. M. Gattie and G. E. Wainwright, gained a rather unexpected victory over their more mathematical opponents. The score was:—

OXFORD.		CAMBRIDGE.	
1. C. D. Locock (University Col.) (unfinished)	—	*v.* Rev. A. B. Skipworth (St. Catherine's) (unfinished)	—
2. E. M. Jackson (New College)	$\frac{1}{2}$	*v.* W. H. Gunston (St. John's)	$\frac{1}{2}$
3. Rev. L. W. Lewis (Lincoln)	$\frac{1}{2}$	*v.* J. N. Keynes (Pembroke)	$\frac{1}{2}$
4. H. F. Lowe (Balliol)	1	*v.* W. Deighton (St. John's)	0
5. R. W. Barnett (Wadham)	1	*v.* Rev. J. F. Sugden (Trinity Hall) ...	0
6. E. Anthony (Ch. Ch.)	0	*v.* F. P. Carr (St. Cath.)	1
7. Rev. W. Cooper (Wadham)	1	*v.* E. L. Kearney (St. Catherine's)	0
8. Rev. W. M. Le Patourel (Balliol)...	$\frac{1}{2}$	*v.* W. R. Fisher (St. John's)	$\frac{1}{2}$
	$4\frac{1}{2}$		$2\frac{1}{2}$

On the same evening a combined team of present members of the two Universities, assisted by Mr. E. M. Jackson, suffered a defeat at the hands of a City of London team by 13—7.

On the Thursday following, the Oxford and Cambridge match took place at the British Chess Club. Cambridge were the favourites, and fully justified the fact, not allowing their opponents to win a single game. In the absence of clocks the rate of play was much slower than usual, and only one game was played at each board. The following was the score:—

OXFORD.		CAMBRIDGE.	
1. D. Madgavkar (Balliol)	0	*v.* H. E. Atkins (Peterhouse)	1
2. R. Lynam (non. col.)	$\frac{1}{2}$	*v.* H. S. Bullock (Corpus)	$\frac{1}{2}$
3. F. E. Jelly (Magdalen)	$\frac{1}{2}$	*v.* F. G. Scovell (Queen's)	$\frac{1}{2}$
4. A. B. Hinds (Ch. Ch.)	$\frac{1}{2}$	*v.* E. Young (Corpus) ...	$\frac{1}{2}$
5. G. A. Heginbotham (Pembroke)	0	*v.* W. C. Sandford (Queen's)	1
6. P. L. Osborn (Magdalen)	0	*v.* E. B. James (Caius)	1
7. P. Sergeant (Trinity)	0	*v.* J. H. Percival (Trin. Hall)	1
Total	$1\frac{1}{2}$	Total	$5\frac{1}{2}$

* The game on No. 3 board was adjudicated by the umpire (Mr. James Innes Minchin) as drawn.

The usual banquet and smoking concert followed in the evening at the British Chess Club, the Lord Mayor being in the chair. On the following afternoon the Oxford Club defeated a rather weak team of the St. George's Chess Club by 5½ to 2½. In the evening Cambridge, assisted by several old members, gained a victory over the British Chess Club by 7½ to 4½, the latter club playing a rather weak team, and losing on the four last boards.

Finally, on the evening of the boat race, the Sussex Chess Association, by no means playing their full strength, defeated a combination of present members of the two Universities by 10—6.

COUNTIES' CHESS ASSOCIATION PROBLEM TOURNAMENT.

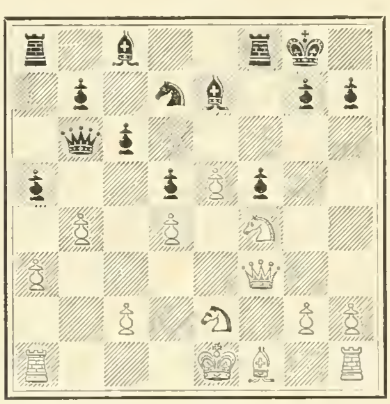
First prize, £2 2s.; second prize, £1 1s. The time for sending in is extended to 30th June, 1892. For full particulars write to Rev. A. B. Skipworth, Tetford Rectory, Horncastle.

The following fine game was played in the late tournament:—

[Vienna Opening.]

White (J. H. Blackburne).	Black (E. Lasker).
1. P to K4	1. P to K4
2. Kt to QB3	2. Kt to KB3
3. P to B4	3. P to Q4
4. P takes KP	4. Kt takes P
5. Q to B3	5. P to KB4 (*a*)
6. Kt to R3 (*b*)	6. P to B3 (*c*)
7. Kt to K2 (*d*)	7. B to K2
8. P to Q3	8. Kt to B4
9. P to R3 (*e*)	9. Castles
10. B to K3	10. QKt to Q2
11. B takes Kt	11. Kt takes B
12. Kt (R3) to B4	12. Q to Kt3 (*f*)
13. P to QKt4 (*g*)	13. Kt to Q2
14. P to Q4	14. P to QR4!

Black.



White.

15. R to QKtsq	15. P takes P
16. P takes P	16. B takes Pch
17. P to B3	17. B takes Pch
18. Kt takes B	18. Q takes P
19. Kt to K2	19. Q takes P
20. Q to B4	20. Q to B3
21. Kt to Q4	21. Kt to B4!
22. Q to Q2 (*h*)	22. Kt to K5
23. Kt takes Kt	23. BP takes Kt
24. Q to K3	24. R to R5
25. Kt to B2 (*i*)	25. R to R7
26. R to Bsq	26. B to Kt5
27. B to K2	27. B takes B
28. K takes B	28. Q to Kt7
29. K to Qsq	29. Q to Kt4
30. R to Ksq	30. R to Kt7 (*j*)
31. Q to K2 (*k*)	31. Q to Q6ch
32. Q takes Q	32. P takes Q
Resigns.	

Notes.

(*a*) 5. . . QKt to B3 is another good defence. White's best reply is 6. B to Kt5: for if 6. Kt×Kt, Kt to Q5; 7. Q to Q3, P×Kt; and he dare not take the Pawn on account of 8. . . B to KB4.

(*b*) If 6. P to Q3, Kt×Kt; 7. P×Kt, P to Q5! with a good game. Or if 6. P×P *en passant*, Kt×P; 7. P to Q4, B to QKt5, followed by Castles.

The best move according to Bardeleben is 6. KKt to K2, P to B3; 7. Kt to B4. Mr. Blackburne's move is not so good:—

(*c*) For here Black should surely play 6. . . QKt to B3, a move which, if the White Knight had played to K2, would of course be met by P to Q4.

(*d*) 7. Kt to B4 would now give White the superior game. The opening is rather indifferently played on both sides.

(*e*) Not quite unintelligible, for if 9. B to K3 at once, Q to Kt3! and White can neither Castle comfortably nor play R to QKtsq without losing a Pawn.

(*f*) Best probably; though P to KKt4 is also tempting, White's best reply being P to Q4.

(*g*) This weakens his game terribly; but R to QKtsq is also disadvantageous. After this Mr. Lasker does not give his opponent a chance.

(*h*) 22. B to K2 would be met by Kt to K3!, winning a piece.

(*i*) Kt to Kt3 may be a shade better, but the game is lost anyhow.

(*j*) In order that the Knight may not be able to attack the Rook after the contemplated exchange of Queens.

(*k*) This loses right off, but there is not much to do. On his next move, if 32. Q to Q2, R to B7 wins a piece by force.

Contents of No. 78.

TERMS OF SUBSCRIPTION.

"Knowledge" as a Monthly Magazine cannot be registered as a Newspaper for transmission abroad. The Terms of Subscription per annum are therefore as follows:—To any address in the United Kingdom, the Continent, Canada, United States, Egypt, India, and other places in the Postal Union, the Subscription is 6 shillings, including postage; or 1 dollar 50 cents; or 6 marks; or 7 francs 50 cents.

For all places outside the Postal Union, 5 shillings in addition to the postage.

Communications for the Editor and Books for Review should be addressed, Editor, "Knowledge" Office, 326, High Holborn, W.C.

KNOWLEDGE

AN ILLUSTRATED

MAGAZINE OF SCIENCE

SIMPLY WORDED—EXACTLY DESCRIBED

LONDON: JUNE 1, 1892.

CONTENTS.

A LUMP OF CHALK AND ITS LESSONS.

By R. Lydekker, B.A.Cantab.

PROBABLY all Englishmen—certainly all those dwelling in the eastern and south-eastern counties—are familiar with the pure white rock which we call, from the Latin *creta*, Chalk. It is indeed this very familiarity which breeds the proverbial contempt, and causes us to take but scant or little notice of what is really a very beautiful substance in itself, altogether apart from the interest with which it is invested from a geological point of view. If Chalk were very rare instead of being exceedingly abundant, there is little doubt that it would be reckoned as a beautiful substance, worthy to stand as the best example of a pure white mineral alongside of virgin sulphur as the finest sample of a yellow one. If, moreover, Chalk had happened to have undergone the action of intense heat under equally intense pressure, it would assuredly have produced an even finer and purer statuary marble than that of Carrara, and might thus have been one of the most valuable of rocks.

A complaint may not unfrequently be heard among those more or less deeply interested in geological science who happen to dwell in a Chalk district, that the very sameness of the Chalk formation throughout England prevents them from finding any interest in the geology of their own districts, and thus leads them to regret that their lot had not been cast in regions where a variety of rocks are to be met with. Although there is a considerable amount of truth in this complaint, yet if rightly studied the Chalk is so peculiar and unique a formation as rather to embarrass us with the number of considerations and problems to which it gives rise, than to be deficient in interest.

Examining a lump of the pure white Chalk of many parts of England, such as that of Dover, we find that it consists, both to the naked eye and under an ordinary lens, of an exceedingly fine-grained homogeneous soft substance, adhering strongly when applied to the tongue, and leaving a white streak when rubbed on other substances. If treated with vinegar, or any other acid, it will effervesce strongly with the liberation of the gas commonly known as choke-damp, or carbonic acid, while the base unites with the new acid to form a fresh compound of lime. The lime may be obtained in a pure condition by burning the Chalk, as in a lime-kiln, when the carbonic acid is likewise given off; and we thus learn that Chalk consists of carbonate of lime. As a rule, when we examine a Chalk-cliff we shall find that the Chalk, although stained here and there with iron, is identical in structure throughout great thicknesses, and that it shows nowhere any signs of crystallization. Occasionally, however, as at Corfe Castle, near Swanage, in Dorsetshire, we shall find that the Chalk has become so hard as to leave no distinct streak when rubbed lightly on other substances; while its cracks and fissures are filled with translucent crystals of white spar—the calc-spar, or calcite of mineralogists. Here then we have the Chalk so hardened, probably by the effects of subterranean heat, as to form what is popularly called a limestone; while a farther step would have converted it into actual marble. The geologists would indeed apply the name limestone to Chalk, ordinary limestone, and marble indifferently; but since the popular usage is different, it is well to be assured that all three are but various modifications of one and the same substance. In the north of Ireland the basalt of the Giant's Causeway has converted the Chalk still more completely into a hard limestone.

Chalk, then, may be defined as a fine-grained, white, non-crystalline, soft limestone. This, however, by no means exhausts the subject of its composition. Thus if we take a piece of Chalk and wash it carefully in water with a hard brush so as to reduce it to a state of mud, and examine the portion which falls to the bottom of the vessel under a microscope, we shall find that this is very largely made up of various shell-like substances. Many of these are minute fragments of what may have been real shells, while others are portions of the spines of sea-urchins, and others, again, are the flinty spicules of sponges. By far the larger proportion consists, however, of perfect objects of extremely minute size, mainly belonging to that lovely group of animals known as foraminifers, or shortly, "forams." Of these beautiful little shells some are coiled in a manner recalling the shell of the nautilus, while others consist of globular masses arranged either in a coil or in a straight line, the globules gradually increasing in size from the summit to the mouth of the shell. In all cases, however, the walls of these shells are perforated by the inconceivably minute apertures from which the forams take their name, and through which, when alive, the creatures protruded delicate threads of the jelly-like protoplasm of which their soft parts are composed. Truly marvellous in beauty are these forams, although pages of description can give but a faint idea of them, and the student should see them for himself under a microscope. So numerous, moreover, are these forams, and other equally minute organisms in the white Chalk, that they frequently compose half its substance, while it is

stated that in some rare cases they may even rise to as much as ninety per cent.

We have now, therefore, to add to our definition of Chalk that it is largely composed of the shells of the minute animals known as forams, together with those of other allied creatures, and so may accordingly speak of it as a limestone which is evidently to a large extent of organic origin. Moreover, as these forams are more or less closely allied to species inhabiting the ocean at the present day, we should be justified from this evidence alone in regarding the Chalk as a formation of marine origin. This origin is, however, equally well proved by the larger fossils, such as shells of sea-urchins, scallops, oysters, etc., commonly occurring in the Chalk; while, in addition to this, the extreme purity and thickness of the formation would of itself be sufficient to demonstrate that the Chalk is the result of long-continued deposition on the bottom of the sea.

Thus much for the composition of our lump of Chalk as examined in the laboratory, and we now turn, as all geologists worthy of the name should, to its occurrence in the field. If we look at one of the tall Chalk cliffs of our southern coasts, as in the neighbourhood of Dover, we shall be first of all struck with the extreme homogeneity and purity of the whole formation from top to bottom, through a thickness which in this neighbourhood is close upon 1000 feet, and in Norfolk is more than 1100 feet. This similarity of composition throughout such a vast thickness is totally unlike what we are accustomed to observe in other rock-cliffs (although there is some approach to it in the blue mountain-limestone of Derbyshire), where we generally find alternating bands composed of rocks differing both in colour and structure from one another, and we are thereby led at once to conclude that there must be something very peculiar connected with the deposition of the Chalk. How was it that in the old sea there were not only no currents bringing loads of sand or clay to alternate with the pure white limestone, but, above all, that there was not a tinge of colouring matter to stain the virgin purity of the newly-forming Chalk during those ages and ages of time, while drifted logs and fruits occur but rarely?

A closer inspection of a large thickness of Chalk will, however, reveal the fact that there is not a complete similarity in the nature of the rock throughout the entire formation. Thus, whereas in places where nearly the whole formation is displayed we find throughout the uppermost 400 feet layers and nodules of flint are thickly distributed throughout the mass, generally forming more or less well-marked lines which indicate the original planes of the deposition of the rock, as we pass to a lower level the proportion of these flints becomes gradually less, till, after we have passed downwards through some 130 feet, they finally disappear, and are wanting throughout the whole of the lower part of the series. Moreover, in this lower Chalk, or Chalk without flints, we shall find, as we pass downwards, a gradual tendency to lose the pure white colour of the upper Chalk, and to assume a buff or greyish tint, while in the very lowest beds we shall not fail to notice the appearance of a number of small grains of a greenish-coloured mineral. If, again, we try to dissolve this lower Chalk in acid we shall find that as we descend in the series there is an ever increasing quantity of an insoluble remnant, which would be shown by analysis to be of the nature of clay. Both these circumstances point to the conclusion that at the time the lower Chalk was laid down the conditions were by no means so well adapted for the deposition of a pure carbonate of lime as was the case in the later time of the upper Chalk with flints. What these conditions were we shall consider subsequently, but we have now to direct our attention to the area over which the white Chalk extends.

In the north-west the furthest limits to which the white Chalk extended are found near Belfast, where, as we have said, the rock has been converted into a hard limestone by the action of heat. Although we do not again meet with Chalk till we reach the east and south of England, where it forms large portions of our coast from Dorsetshire to Yorkshire, yet it is probable that the Chalk sea embraced the foot of the Welsh mountains, which formed an archipelago. From England the white Chalk may be traced without any alteration in its character through the north of France, the south of Belgium, the eastern part of the Netherlands, and thence through Westphalia, Hanover, and Galicia, into Poland and Russia, where it reaches on the one side to the foot of the Urals, and on the other to the Crimea; moreover, to the northward it occupies a considerable portion of Denmark and the southern extremity of Sweden. Although the white Chalk is now only distributed over the surface of this region in larger or smaller patches, being sometimes covered up by newer (Tertiary) deposits, and in other places totally wanting, there is evidence that it once extended continuously over the whole. Moreover, the absence of any traces of the white Chalk in the regions to the west and north of those mentioned, indicates that the present limits of the Chalk in those directions mark approximately the boundaries of this cretaceous sea; this sea being probably cut off from free communication with the Atlantic by a barrier connecting western France with Cornwall and Ireland, and by another joining Scotland with Scandinavia.

The above area includes the whole of the white Chalk; but when we trace this Chalk southwards into Bohemia and Saxony we find that it has undergone a very remarkable change. Thus, although it contains the same fossils as to the northward, the rock itself, instead of being the pure white limestone to which we have been accustomed, consists of a series of massive sandstones about as unlike Chalk as anything well could be. It is probable, indeed, that these cretaceous sandstones, as we may call them, were formed in a gulf on the southern coast of the white Chalk sea, which was unfavourable to the deposition of Chalk itself; and as these sandstones were undoubtedly deposited at the same time as the pure Chalk, we thereby learn the very important geological lesson that similarity or dissimilarity in the mineralogical structure of a rock is a matter of very minor import indeed. We may illustrate this by reference to architecture. Thus, a Gothic church may be built either of sandstone, limestone, marble, or, for the matter of that, brick; but it will still be (exclusive of course of our so-called modern Gothic) absolutely characteristic of one particular period of European architecture. This Gothic style will be distinguished by certain peculiarities in the structure of its arches and pillars, as well as by the ornaments with which they are embellished. Just so in geology we have a Chalk or cretaceous style, in which, although the rock itself may be either Chalk or sandstone, or limestone, or slate, yet its architectural details—that is to say, its fossils—will be the same, not only throughout Europe, but within certain limitations of variation, over the whole world. This is one of the important lessons to be learnt by a comprehensive study of our white Chalk.

The second great lesson taught by the white Chalk is, however, of perhaps still more importance. We have seen that the white Chalk was deposited in a sea cut off from free communication with the Atlantic to the west and north; and the range of the Ardennes which formed its

shore in the south-west, together with the evidence of the near neighbourhood of a coast afforded by the sandstones of Saxony and Bohemia, indicates that this sea was a *mare clausum* (in a geographical, not a political sense), somewhat like the Mediterranean or the Black Sea. Now, from the apparent similarity of Chalk to the ooze forming in the abyssal depths of the Atlantic and the other large ocean basins, it was taught but a few years ago that the Chalk itself was deposited in an ocean of similar depth. The *mare clausum* theory, however, is of itself a sufficient obstacle to the acceptance of such a view, since it is impossible to conceive that a sea of such small dimensions could ever have had depths at all approaching those of the Atlantic. The Atlantic theory, if we may so call it, of the Chalk was, however, at once and for ever dissipated by the researches carried on during the voyage of the "Challenger." Those researches showed that the so-called abyssal deposits, instead of being very like the Chalk, were really very different. Even the ooze has not the purity of the Chalk; while the large areas of red clays covering the ocean basins have no analogy in the latter. Moreover, it has been proved that the abyssal deposits are laid down at a rate of almost inconceivable slowness—so slowly indeed that even meteoric dust forms an appreciable portion of the red clays; while the ear bones of whales and teeth of sharks that strew the ocean floor have lain there so long as to have become coated over with a thick layer of manganese precipitated from the water of the ocean. On the other hand, the remains of fishes and other delicate organisms which occur so beautifully preserved in the white Chalk clearly indicate that its deposition must have been comparatively rapid, and must have taken place in a sea where there was abundance of mineral matter either in suspension or solution. Again, the fauna of the Chalk, especially the sponges, is one such as would be found in comparatively shallow seas, and is quite unlike that of the Atlantic depths. Indeed, it is quite probable that the Chalk sea may not have exceeded some one to two thousand feet in depth. The great difficulty in regard to the Chalk is, indeed, to explain its purity, and the very rare occurrence of drifted materials found embedded in it. The *mare clausum*, with no tides and perhaps but few large rivers flowing into it, and its shores largely composed of hard crystalline rocks like those of Scandinavia and the Ardennes, will, however, to a certain extent remove this difficulty. Even then, however, it is doubtful how sufficient material for the formation of the Chalk could have been obtained; and accordingly one of our most eminent living geologists suggests that, in addition to its partially organic origin, Chalk may have been largely formed by a chemical precipitate of carbonate of lime.

Be this as it may, the degradation of Chalk from its former position as a supposed typical abyssal deposit has taught the great lesson that almost all the stratified rocks with which we are acquainted were laid down in comparatively shallow water, and consequently has led to the general acceptance of the grand doctrine of the permanence of continents and ocean basins. By this, of course, it is not meant that the whole areas of several of our continents, such as Europe, have not been (as we know they have), many times over, beneath the sea. Indeed, what we have already said as to the extent of what we may call the cretaceous Mediterranean, shows that at a comparatively late period of geological history a large part of central Europe was sea. Neither does this doctrine forbid such changes in the present configuration of the earth as would be implied by a land connection between Africa and southern India. What, however, it does say, and that in the most emphatic manner, is that where continents now are there deposits have always been going on, and there land, of larger or smaller extent and of ever varying contour, has always been; while the great ocean basins, like those of the Atlantic and Pacific, have existed as such since the globe emerged from its primeval chaos. This, then, is the second great lesson taught by a lump of Chalk!

We have, however, by no means yet exhausted the interest connected with the subject of Chalk. In the first place, the gradually increasing marly character of the lower Chalk points to a condition when the sea was much less deep than at the period of the white Chalk. If, indeed, we go lower down in the rock series, we shall find the white character of the Chalk has completely disappeared when we reach the underlying blue "gault" of Folkestone, which implies the existence of currents or rivers largely charged with mud. Still further back, we have the freshwater clays and sandstones of the Weald of Kent and Sussex; and we thus learn that at that period southern England was in the condition of a large delta, after which there was a gradual subsidence, culminating in the *mare clausum* of the period of the white Chalk. Then, again, we have seen how the "architectural style" of a rock, as exemplified by its fossils, is the one all important point connected with it; and the alteration of the English Chalk into the cretaceous sandstones of Saxony ought to have prepared us for more extensive modifications of these rocks as we proceed to regions still more remote from where they are typically developed. If, then, we turn to a geological map of Europe, we shall find a large area of its southern half coloured in, as being formed of cretaceous rocks—that is, rocks equivalent in point of age to the white Chalk. The description, or still better, an actual examination of these rocks will show, however, that they have but little in common with the white Chalk. They consist, indeed, of hard, compact, and often dark-coloured limestones, containing many fossils identical with those of our own Chalk, together with certain others of different types; thus showing that we have entered an area where the conditions of life were somewhat different from those obtaining in the *mare clausum* of the white Chalk. From the centre and south of France these cretaceous limestones may be traced across the Pyrenees into Spain, and so into North Africa, while eastwards they extend across the Alps into Switzerland, Italy, Bulgaria, Roumania, and thence along the Mediterranean basin into Asia. That these rocks stretch far into the heart of Asia is now well known, and since rocks of somewhat similar type containing well-known European cretaceous fossils are found in the inner Himalayas, it seems highly probable that this southern cretaceous sea connected the Mediterranean with the Bay of Bengal. Whereas similar cretaceous fossils occur on the east coast of India, in the neighbourhood of Madras, and since there are some very remarkable similarities between the freshwater rocks of the peninsula of India and those of South Africa, while many animals are now common to those two countries, there are very strong reasons for considering that peninsular India (which was then cut off from the rest of Asia by the cretaceous sea) had a land connection with the Cape by way of Madagascar. We know indeed that this southern cretaceous sea communicated freely with the Atlantic, by what is now Spain and France, and we are thus led to conclude that there was formerly a direct sea communication between the Atlantic and the Bay of Bengal by way of central Asia. Europe and Asia then formed a northern continent separated by this cretaceous sea (of which the Mediterranean is the shrunken remnant) from a southern continent which included both Africa and India proper.

Such is the wide interpretation given to the doctrine of the permanence of continents and ocean basins.

The study of the European Chalk, besides the two great lessons to which we have especially directed attention, has, therefore, proved to us the former existence of two great seas, in which the cretaceous rocks were deposited—the northern one being a *mare clausum*, cut off from the Atlantic, in which was deposited the white Chalk; while the southern one, in which the hard, massive limestones of southern Europe were laid down, formed the connecting link between the Atlantic and the Indian Ocean, to which we have already alluded. We might pursue our subject further, and discuss the origin and nature of the flint and pyrites which are of such common occurrence in the Chalk, or we might direct attention to the more valuable and much rarer phosphates which are sometimes contained in it. We might, again, discuss the peculiar characters of the cretaceous fauna, and show how that of the closed northern sea differed from that of the open southern ocean. We might do all this, and more; but what has been written is sufficient to show the amount of interest and the many weighty problems connected even with a "Lump of Chalk."

ANTS' COMPANIONS.—II.

By E. A. Butler.

Unquestionably the most curious of all the beetles that associate with Ants is the little *Claviger foveolatus* (Fig. 2), which lives specially, though not exclusively, in the nests of the Yellow Ants (*Lasius flavus*). It is a shining, hard-bodied, reddish yellow insect, no more than one-twelfth of an inch long, with a broad abdomen which carries a deep pit in its centre, but a small, narrow, and almost rectangular head. Though belonging to a different family, it in some respects resembles the Brachelytra, of which we spoke last month, inasmuch as it has, like them, short elytra covering only the base of the abdomen; but there are no wings underneath these, the insect being apterous and unable to fly. It is also mutilated in other respects; for instead of the pair of claws with which each foot of an insect usually terminates, only a single one is present on each. The small size of the head, again, is partly due to the absence of eyes, for the beetle is quite blind; but, apparently to compensate for the loss of this one function, the other organs of sense, the antennæ, are exceptionally large, thick and club-shaped, suggestive of a high development of whatever sense, whether of smell, touch, or any other, resides in them. Further, the beetle seems to have lost the power of feeding itself, for its guardians feed it in the same way as their own young, and its mouth organs are quite rudimentary. Here then we have an insect which has lost the powers of flight and sight and of helping itself to food, and which would plainly be doomed to speedy extinction but for the fostering care of the Ants. We have spoken of its having *lost* the above powers, advisedly; for though there is of course no *direct* evidence on the subject, yet when we compare the insect point by point with other members of the same family which do not make it the rule of their life to practise parasitic habits, and are in full possession of all their powers except that of flight, and when we see how exceptionally well developed their eyes and feeding apparatus are, it seems impossible to resist the conclusion that in *Claviger*, which, while unlike them in these respects, closely resembles them in others, we have an instance of the suppression of parts through disuse. The family in question is called *Pselaphidæ*, and is represented by a large number of small species in different parts of the globe; in this country we have a little more than thirty kinds, all minute insects, *Claviger* being one of the largest. Many of them are common, and they may be found in moss, under stones, amongst dead and decaying leaves, in refuse heaps, &c. They have strong, sharply-notched jaws, and are carnivorous in habits, being supposed to feed chiefly on mites. Fig. 3 shows the head of one of the commonest of the *Pselaphidæ*. In it we may specially note the extremely prominent, mulberry-like eyes, and the very long maxillary palpi, which look like a second pair of antennæ, in extraordinary contrast to the mutilated condition of the head of *Claviger*. This is the condition of most of the *Pselaphidæ*, and it shows what might have been expected in *Claviger*, but for its parasitic habits. It will be observed that, though *Claviger* has very stout antennæ, the number of joints is much less than in the rest of the family.

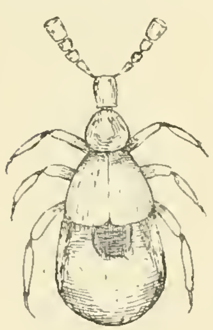


Fig. 2.—*Claviger foveolatus*, a blind beetle found in Ants' nests; magnified fourteen diameters.

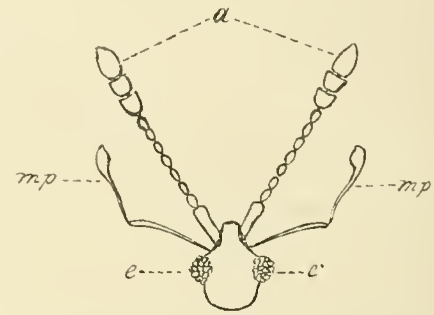


Fig. 3.—Head of *Pselaphus Heisii* for comparison with *Claviger*. *a* antennæ, *mp* maxillary palpi, *e* eyes.

This remarkable little beetle carries at the outer corner of its elytra certain tufts of yellow hairs, which the Ants have been observed to take in their mouths and lick, sometimes continuing the operation for eight or ten minutes at a stretch. The hairs apparently yield some kind of secretion which is agreeable to the Ants' sense of taste, and thus one can easily understand that "cupboard love" will operate strongly in the direction of inducing the Ants to take the greatest care of their interesting charges. The extreme importance of these tufts of hairs and their secretion to the little beetle is evident; its life, as a species, literally hangs on these threads, and this very fact tends in the direction of improving the organs, and thus securing to the species a more certain tenure of life. The beetles are found in those nests of the Yellow Ant which are constructed under stones, especially in the chalky districts of the South of England, but apparently not in those that are under turf. The beetles are attended by the Ants, and carried about from place to place as occasion requires, though of course they can walk themselves if they choose; their pace, however, is but sluggish.

We have already pointed out that *Claviger* is not to be found in all the nests of the Yellow Ant; the same remark holds good of the other species of Ants with which it may be associated. Lespès has made the observation that specimens of the beetle which had been removed from a nest of the common Garden Ant (*L. niger*), on being transferred to another nest belonging to Ants of the same kind, but in which no such pets were kept, were destroyed and eaten, instead of being cherished. He concluded that this was an indication of their failure to comprehend the use of the beetles, and that therefore different communities

amongst the same species of Ant had reached, so to speak, different degrees of intellectual development on the subject of the domestication of alien insects. Sir John Lubbock's experiments in connection with another pet, which we shall consider more in detail presently, seem, however, to throw some doubt on such a conclusion. *Claviger* is not the only member of the *Pselaphidæ* which may be found in Ants' nests; several others may also be met with occasionally, but they have not the degraded habits and structure of *Claviger*, and their occurrence in the nests is not therefore a matter of prime necessity to themselves. One species, called *Batrisus venustus* is a rare insect, and is said only to occur *singly* in the nests.

Another Ants'-nest beetle is figured in the adjoining illustration (Fig. 4). It is one of the Brachelytra and a member of a genus (*Myrmedonia*), all the species of which are associated with Ants. They do not, however, appear to be on such intimate terms with their hosts as the beetles already mentioned, and indeed it is considered by some observers that the Ants, so far from cherishing them, regard them with distrust and suspicion. They are all brownish or blackish insects, larger than those already mentioned, of fragile structure and active habits. Many other of these myrmecophilous (Ant-loving) beetles might be illustrated with advantage; indeed, a complete portrait gallery of the whole company would be an interesting collection, revealing, as it would, the wonderful diversity of form, colour, and size which marks these immigrants into Ant territory. But we must pass on to other branches of our subject.

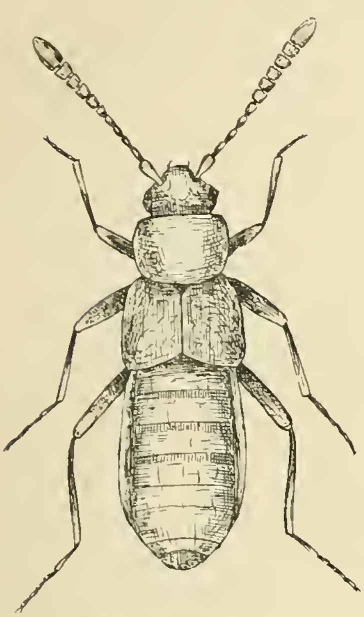


Fig. 4.—*Myrmedonia funesta*, a beetle found in Ants' nests; magnified ten diameters.

The larvæ of beetles are sometimes found in Ants' nests, though the perfect insects live elsewhere. Thus the brilliant rose-beetles or rose-chafers, which, in their perfect condition, may be found in the flowers of roses, proceed from fat whitish grubs, which are to be met with in the heaps of the mound-building Ants. No greater contrast could be imagined than between the soft, ugly, fleshy grub and the hard-skinned handsome beetle, which, as it rests embosomed in rose petals, with the sun shining on its brilliant golden green or coppery back, is a perfect gem of loveliness. In the Ants' mounds the grubs find a comfortable home as well as a means of subsistence, while one species has been accused of making a traitorous return for the shelter it receives by devouring the Ants' "eggs." Similar habits pertain to the larvæ of another beautiful beetle of very different structure and anatomical relations, called *Clythra quadripunctata*; its specific name, signifying the "four-dotted," refers to the four black spots that conspicuously mark its long yellowish red elytra; the rest of its body is black. It is a fine-looking insect, about $\frac{3}{8}$ths of an inch long, and its larva inhabits, not very commonly, the nests of the Wood Ant (*F. rufa*), living in a hairy, leathery case, which it drags about, while its head and legs protrude from one end.

The order Hemiptera, or bugs, furnishes its quota of foreign residents in Ants' nests. Some of these bear a superficial resemblance to the Ants; others are quite unlike them, and their presence is probably sufficiently accounted for by the attractiveness of the shelter which the piles of bits of stick, grass, &c., afford. To the same order belong the aphides, which are among the most remarkable of all the Ants' companions; to these we shall recur presently. Of the order Collembola—the springtails, which are so abundant under stones and decaying logs—Sir John Lubbock mentions one named *Beckia albinos*, which exists in large numbers in some Ants' nests. It is a minute insect of an active temperament, like most of its allies, and its leaping power depends on a kind of forked tail bent under the body; this, on being struck on the ground, projects the insect into the air, repeated blows causing it to skip about in a promiscuous fashion, apparently without any very definite idea of where it is going to be landed. These little white creatures run in and out amongst the Ants, which, however, seem to be totally oblivious of their presence, and hence its association with them is probably accidental, arising merely from similarity of habitat. Like *Claviger* they are blind, and would therefore seem to find their way about by means of their antennæ, which are kept in a state of perpetual vibration. Notwithstanding this defect, they are dainty little creatures, and are very particular about their personal appearance, frequently tidying themselves up, and being especially careful about keeping their feet clean. The feet are furnished with comb-like claws, the action of which would of course be impeded by any accumulation of dirt.

A little white woodlouse (Fig. 5) is another interesting Ants' guest. In the association of this creature with Ants there is something more incongruous than in all the other cases we have mentioned, for all these have been associations of insect with insect, though the guests are not of the same order as their hosts; this is a parallel case to the keeping of dogs and cats by human beings, where we have mammal with mammal, though of different orders, Carnivora with Primates. But woodlice are not insects, as their numerous legs, among other characteristics, attest; they belong to the class Crustacea, which contains also crabs, lobsters, shrimps, barnacles, water fleas, and numberless other creatures, and they are some of the chief terrestrial representatives of the class. In their association with Ants, therefore, we find a parallel, not to the instances of domestication referred to above, but to cases in which human beings keep tortoises, lizards, toads, or newts, as pets, and the zoological interval between the Ant and the woodlouse is a wider one than between the Ant and the beetle, the bug, or the springtail. As a group, woodlice are perhaps best known to the majority of persons by a *slate-coloured* representative called the Armadillo woodlouse, which is excessively common in gardens and elsewhere, under stones or under the bark of dead trees, and which has the habit of rolling up into a ball when disturbed, a habit which is not, however, common

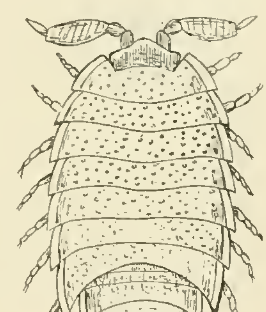


Fig. 5.—*Platyarthrus Hoffmanseggii*, a blind woodlouse found in Ants' nests; magnified ten diameters.

to the group. The Armadillo woodlouse must, by the way, be distinguished from another common creature which superficially resembles it, and which exhibits the same peculiarity of rolling into a pill-like ball; which also is found in similar situations, and especially in the wood of decaying tree-stumps. It is not, however, slate-coloured, but of a deep shiny blackish brown, and the minuteness and disposition of its legs, amongst other less obvious characteristics, pronounce it a member of the class Myriapoda, which contains the centipedes and millipedes, instead of the class Crustacea. It is popularly known as the pill millipede. To the woodlice, then, which are Crustacea, and not to the millipedes, belongs the milk-white Ants' companion above mentioned; but, though we have several species of woodlice in the British Islands, and several of them remarkably common, this is the only one that takes up its quarters in Ants' nests. It is dignified with the extraordinary name *Platyarthrus Hoffmanseggii*. On lifting the stone which covers a nest of the Yellow Ant, we may often see numbers of these little creatures slowly crawling about, like little white scales, very conspicuous by their clean and bright appearance, which contrasts sharply with that of their hosts. What their business in the nests is, remains a mystery, and as the Ants take no more notice of them than they do of the springtails above mentioned, it is evident that they do not, like *Claviger*, form a secretion agreeable to their hosts; but whatever the bond of association may be, it seems to be one of long standing, for while other woodlice have good eyes, *Platyarthrus* has become blind, a very natural result of living long in darkness, in the subterranean galleries of the Ants. In this blind woodlouse we see the same apparent compensation of senses as in *Claviger*, for the antennæ, like those of the blind beetle, are remarkably broad. These white woodlice do not possess the power of rolling into a ball. They have been found living in the nests of at least six different kinds of Ants, and according to Sir John Lubbock's observations, when they were transferred from their native nests to others previously unoccupied by the species, they were received without molestation.

We have now to notice what is perhaps the most remarkable part of our whole subject, the association of Ants with aphides or plant lice. The fondness of Ants for sweets is at the bottom of this association. Aphides secrete the sweet sticky fluid called honey-dew; this, Ants eagerly sip up, and this passion of theirs has led them to the adoption of ingenious devices to ensure a constant supply. This is, however, merely a general statement, and must not be taken as implying either a uniformity of procedure on the part of all Ants, or an identity of treatment of all species of aphides; in the details of their relations, as in other matters, we see well illustrated the variety of Nature. It is an oft-repeated observation that the Garden Ant climbs shrubs and bushes in search of the aphides that frequent twigs; but one would not expect such species as the Yellow Ant, for example, to do this; their time is chiefly spent below ground, and whatever aphides they have to do with will be found either in or near their own nests, and therefore on low plants. The tree-infesting aphides have each a pair of minute tubes projecting from the hinder part of the abdomen, and out of these issues the glutinous secretion, while its flow is accelerated when the Ants tap the body of the aphis with their antennæ. Some Ants get no farther than this; such would live chiefly on what they can get in their foraging expeditions, making no provision in their own nests for a continuous supply of food of any kind, subsisting, as it were, from hand to mouth. Others, however, have reached a more advanced stage; conscious of the value of the aphides as yielding a continuous supply of an agreeable article of diet, they in some cases build earthen sheds over them, as well as protecting them by their own individual exertions from the attacks of enemies. Others, again, have got still farther than this; not satisfied with protecting the mature aphides, they take care of the eggs also, keeping them in their nests through the winter with provident sagacity, securing thus the safety of their next year's supplies. The observations of Sir John Lubbock in this connection are very remarkable. The Yellow Ant was again the species observed. In a nest of these insects he found on one occasion in the month of February some dark eggs, which were those of the aphides; when the nest was disturbed the Ants manifested great anxiety over these eggs, and carried them down into a place of safety. Some of these eggs he took home and offered to some of his own Ants; they were immediately recognised by the Ants and carried into the nest; in about a month's time they were hatched and yielded young aphides. But now came a remarkable result: the young aphides, instead of stopping in the nest, a result that might fairly have been expected to follow, either came out of their own accord or were carried out by the Ants; not finding outside, however, such a food plant as they wanted, they soon died. Some other experiments were equally unsuccessful, but at length the truth of the matter was accidentally discovered. In this last experiment there happened to be near the nest some wild plants, such as usually grow near Ants' nests, and when the aphides were hatched they were taken by the Ants straight to this spot and placed on the leaves. Then, shortly afterwards, others similar to these, and either brought out by the Ants or of independent origin, were found on a daisy plant near; the Ants valued them sufficiently to run up a wall of earth around them, and thus the whole set continued flourishing throughout the summer. In the autumn they laid eggs similar to those that had been found in the Ants' nests. So the secret was out. The Ants took charge of the eggs during the winter, when they were of no use to them, but when, if left outside, they would have been exposed to all the vicissitudes and rigours of our English climate. When spring arrived and the eggs hatched, and the insects were becoming useful to their guardians, they were actually carried out by these and placed on plants near the nest where they could enjoy suitable food, and where also they would be easily accessible to their owners, who to protect their property proceeded to construct works and enclose them in a suitable preserve. This certainly looks like very marvellous intelligence.

But besides these aphides which are supported on the leaves of living plants outside the nests of the Yellow Ant, several species are also habitually found inside the nests. It is not altogether clear, however, what is the relation between these aphides and the Ants. There is a whole section of the tribe of aphides, which, instead of attacking the leaves and young shoots of plants, live upon juices obtained from their roots, and therefore necessarily dwell under ground. They differ a good deal from the rest of the tribe; for example, no winged forms are known in connection with them, and they are destitute of the two tubes above referred to as honey taps, and their bodies are covered with a waxy substance which exudes as a gummy secretion from certain small openings on the back. This substance seems intended to protect the insects from the damp of the soil by which they are surrounded, as well as to cover their eggs. Now, most species of this group are found in Ants' nests, but since the loose sandy soil and the warm situations chosen by the Ants are also just those that suit the aphides, it does not follow that the association is

more than an accidental one caused by similarity of habitat. Mr. Hardy, speaking of one of these root-feeding species, says, "When *Forda formicaria* (the aphis) prevailed in the nests of *Formica fuliginosa*, I noticed that the Ants paid no attention to *them* when the hillocks were disturbed. The aphides slowly re-covered themselves with earth, and those which failed to do so were left quite unnoticed by the numerous Ants running about them." In other cases, however, the aphides were carried off by the Ants, a good deal of persistency being manifested by them in doing so. Mr. Buckton surmises that these wingless subterranean aphides may ultimately turn out to be secondary forms (dimorphs) of species whose winged forms are found above ground.

Aphides are not the only insects whose glutinous secretions are palatable to Ants. M. Nicéville, in the "Journal of the Bombay Natural History Society," describes the caterpillars of certain butterflies belonging to the *Lycænidæ*, found in India, which secrete a sweet liquid from an oval opening on the back of the eleventh segment of their body. The Ants are fond of this liquid and excite its flow by stroking the caterpillars with their antennæ. They are said to arrange a sort of earthen nest, with stalls in it, at the foot of the tree on which the caterpillars are feeding, and when the caterpillars are about to pupate, to lead and drive them into this, so that they may be protected when in their helpless pupahood. When the time comes for the issue of the butterfly, it is helped out of its shell with tender care by its guardians, but if it should prove a cripple, they bite off its wings and carry the body into their nest, possibly to use as food. Other insects, also, especially some belonging to the Homoptera or Frog hoppers, are valued by Ants for secretions which they supply.

To conclude then, it appears from what has been said above, that the relations between Ants and other insects are of the most varied and complex character; the more they are studied, the greater seems to be the variety. Much yet remains to be discovered—in fact, the unknown probably far exceeds the known—so that here is a tempting field of observation for anyone who has the courage and opportunity to enter upon it. M. Carpentier has very well summarized some of the chief facts so far as at present known, and with an extract from his summary we will close this paper: "Some insects live side by side with Ants of all kinds, because they have the same habitat, the bark of fallen trees, old stumps, flat stones, moss, &c. Mutual forbearance is then the order of the day; and the necessary neighbourly feelings are maintained, while at the same time the rights of all are respected. But other insects are attracted by the kitchen stores of the Ants, by their refuse matters, by the different materials that they use in their building operations, or by other unknown causes, whether casual or constant. They may also be born in the Ant-hills, having been brought in unintentionally by the Ants in one of their earlier stages, along with the provisions which they go out to collect from a distance."

PHOSPHORUS MIRABILIS.

By Vaughan Cornish, B.Sc., F.C.S.

ANY substance capable of shining in the dark was originally termed *a phosphorus*. Several substances having the property were known as early as the middle of the seventeenth century, such, for instance as barium sulphide, the "Bonnonian Phosphorus," to which may be added the sulphides of calcium and strontium. The power of barium sulphide and similar bodies to *phosphoresce* depends upon their being previously exposed to light. When a ray of light falls on any substance, part is reflected and part absorbed. As long as the body is exposed to the ray of light it is itself a source of luminous disturbance, and consequently a visible object. The peculiarity of substances such as the sulphides of the alkaline earths is that they continue to be a source of luminous disturbance for a time, when no longer exposed to the ray of light. This power of a body to store up, and slowly dole out the luminous vibrations it receives is called, in physical optics, *phosphorescence*.

Bodies having this property are termed phosphorescent bodies.

Of all substances luminous in the dark, common yellow Phosphorus is the best known, and the example which most people would cite as that of the typical phosphorescent body. Singularly enough, the causes which induce the glow of the chemical element Phosphorus are altogether distinct from those we have mentioned as the cause of phosphorescence in barium sulphide and similar bodies. The terminology of the subject has undergone a peculiar alteration since the seventeenth century. At that time any substance capable of shining in the dark was called "a Phosphorus." Now, the name Phosphorus is restricted by chemists to one chemical element. The element in question exists in more than one allotropic modification, and one of these forms (red, or amorphous, Phosphorus) does not shine in the dark.

While, on the one hand, chemists have made the term Phosphorus special, instead of generic, physicists usually apply the term phosphorescent to a whole class of substances; but "Phosphorus" does not belong to this class.

In the present article we will give a short sketch of the investigations which have been made into the subject of the "Glow of Phosphorus." Ordinary yellow Phosphorus was first prepared by an alchemist of Hamburg named Brandt, and in spite of the care with which the secret of its preparation was guarded a number of persons soon became possessed of the method of manufacture. The early methods, however, gave but a small yield, and were so difficult to carry out that the substance remained for long an extremely expensive chemical curiosity. Its many remarkable properties were a favourite subject for exhibition among the learned and curious, and earned for Brandt's production the name of the *Phosphorus mirabilis*. Robert Boyle observed that the *Phosphorus mirabilis* differed from other shining bodies in that its luminosity did not depend upon its being previously exposed to light. Subsequently it was observed that if Phosphorus were brought into the vacuous space above the mercury in a barometer tube the body no longer shines in the dark. It seemed probable, therefore, that the glow was induced by the presence of air. As in most phenomena in which air takes part oxygen is the active agent, it appeared likely that the glow was due to some action between the Phosphorus and the oxygen of the air. It was found that in pure oxygen Phosphorus at the ordinary temperature and pressure did not glow at all. The glow can, however, be induced either by "partially exhausting" the oxygen in the vessel (*i.e.*, by diminishing the pressure), or by raising the temperature. If after the latter means had been adopted the oxygen were compressed, the glow again disappeared. Now, at the ordinary temperature Phosphorus volatilizes, or evaporates at a very appreciable rate. From these facts, therefore, it seemed probable that the glow was due to an action between the vapour of Phosphorus, and oxygen, the two factors essential for the production of the glow being the presence

of oxygen and conditions favourable to evaporation. The correctness of this conclusion is well shown by the following facts. If Phosphorus be placed in hydrogen, or in carbonic acid, no glow is seen, but traces of Phosphorus vapours can readily be detected in the gas. When one of these gases charged with the vapour of Phosphorus is brought into contact with oxygen gas, a glow is at once observed. This glow is stronger in the case of hydrogen than when carbonic acid is used, which is in accordance with the fact that Phosphorus evaporates more readily in an atmosphere of the lighter gas.

It appears that the glow of Phosphorus in oxygen is in some way connected with the presence of ozone.

It is a well-known fact that when a stick of Phosphorus is placed in moist air, ozone is produced, and it has further been observed that if a drop or two of ether, or oil of turpentine, substances which destroy ozone, be placed in the ozonized air of a vessel containing a piece of the Phosphorus, the glow of the Phosphorus is at once quenched.

It appears probable that the glow is nothing else than a very feeble flame, which may be seen when circumstances are favourable to the oxidation, or burning of the Phosphorus. If the temperature be raised to a moderate degree the combustion takes place with greatly increased energy, and we get the ordinary flame of burning Phosphorus. Recently Professor Thorpe, and other workers at the Royal College of Science, have investigated a similar case of phosphorescent appearance due to oxidation. This occurs with the *trioxide* oxide of Phosphorus.

When Phosphorus is burnt in a rapid current of air, one of the principal products is the trioxide which is capable of combining with a further dose of oxygen, forming the better known and more stable substance, pentoxide of Phosphorus.

The phenomena accompanying this oxidation of the lower oxide (the trioxide) have of late been carefully studied. A phosphorescent appearance is observed when oxidation occurs, and it has been found possible, by varying the conditions of temperature and of pressure, to pass insensibly from the feeblest glow to the most brilliant combustion.

The trioxide is a more volatile body than Phosphorus itself, and better adapted for experiments to show the gradual passage from the "degraded combustion" of the "glow" to the ordinary burning with visible flame.

There are other well-known appearances besides those presented by Phosphorus, which are due to degraded combustion: one example is furnished by the feeble lambent flame seen *inside* the wire gauze of a Davy lamp in "fiery" parts of a coal mine. The conducting power of the wire gauze distributes the heat of the flame over a large area, and prevents the inflammable gas outside the lamp from becoming heated to the point at which explosion occurs.

The *Ignis fatuus* is quoted as a still more striking example of the *degraded* combustion of an inflammable gas. Marsh gases, the slow combustion of which is seen in the feeble flame of the will-o'-the wisp, consist largely of methane, or fire damp, the explosive gas of mines.

Their slow oxidation (or degraded combustion) by the air of the marshes produces at night time a faint glow of uncertain or shifting position, to whose misleading light have been attributed difficulties of the road, as many as have beset the search of scientific men after the true cause of the glow of Phosphorus.

THE NEW STAR IN AURIGA.

By E. W. MAUNDER, F.R.A.S.,

Assistant superintending the Solar and Spectroscopic Departments at the Royal Observatory, Greenwich.

"NEW" or "temporary" stars as they are sometimes called, offer many perplexing problems for solution, and so far we have certainly not arrived at any satisfactory conclusions respecting them. First of all, we have the paradox that they are evidently very minute bodies, for they cool very quickly, quicker than a body even the size of one of the minor planets could do, whilst they are also equally clearly very large bodies, for in no single case has an appreciable parallax been found for any one of them. They are therefore situated at extremely remote distances from us, and hence must be vast indeed for us to perceive them at all when far off.

Their spectra, too, rather confuse than help us. The bright lines of hydrogen, and many of the chromospheric lines familiar to us in the sun, point to a body similar to our sun and the stars of the same class. But then, again, we have other lines shown, which suggest analogies to the Orion stars, supposed, with so much probability, to be but recently formed from the diffused nebulous matrix of that constellation, and on the other hand we have appearances which suggest kinship with the stars of the third type of spectrum, a type usually supposed to represent a cooler, more condensed stage than that even of the sun.

The first difficulty has been attempted to be surmounted by supposing that we have in a "new" star: (1) A momentary flicker on the part of a nearly extinct sun. It has been suggested that the still glowing nucleus is skinned over by a thin non-luminous or feebly luminous crust, that in some way or other this crust is temporarily broken up, and for a time the hidden light and heat are able to radiate themselves forth. (2) Or it is supposed that the increase of brilliancy relates merely to the very surface of the star, and does not denote any general increase in temperature of the star itself. (3) Or that both statements of the paradox are true, and the star is both very minute and very large; or rather that it is composed of bodies individually very minute, but in the aggregate having an immense extension, and considerable mass; in other words, we have not to do with a star in any true sense of the word, but with a stream or streams of meteorites.

How do the facts which we have learned concerning Dr. Anderson's new star accord with these theories?

The first fact that we have is the increase of brightness of the star, from fainter than the eleventh magnitude on November 2nd to brighter than the fifth magnitude on December 20th. The photographs taken by Prof. Pickering are our warrant for this statement. The star therefore increased in brightness one thousand fold in less than seven weeks, quite possibly in even a few days or hours.

The next fact is that the principal source of this added brightness lay in most intensely heated gases. The evidence for this fact is the extreme brilliancy of the bright lines which glowed in every part of the spectrum, from as far down in the red as the eye could reach to the utmost limit of recordable stellar radiations in the ultra-violet. The existence of a bright line spectrum is in itself sufficient evidence that it proceeds from highly heated gas; the existence of groups of such lines in the ultra-violet, as far as lines have been discovered in the spectrum of the mighty Sirius, is the most emphatic testimony we could

have as to the intensely high temperature to which the star was raised. Dr. Huggins has been able to photograph as bright lines the *entire* series of hydrogen lines which he discovered in the ultra-violet spectra of the Sirian stars, and he has also obtained the clearest tokens of the presence of two groups of bright lines, apparently corresponding to, but more pronounced and developed than, two groups of dark lines which he has but recently discovered in the spectrum of Sirius, and which lie far beyond the last of the hydrogen series.

A third fact is perhaps the most striking of all, the most "sensational," if we may be allowed the term. It is that the lines of hydrogen are present both as bright and as dark lines—and the same is true of the lines of some other elements, such as sodium and calcium for example—but that the body or bodies giving the bright lines are moving in the line of sight as compared with that giving the dark lines at the incredible rate of 600 miles per second, or, in round numbers, fifty millions of miles a day!

Clearly, we have not to do here with the breaking up of a cooling crust and its sinking in a molten sea. Equally clearly, no mere temporary brightening of a photospheric surface is in question; and indeed, that idea is scarcely admissible at all in view of the general opinion of the day that photospheres are condensation surfaces. An increase of temperature might raise the height at which that condensation takes place, and so increase the radiation from the particular star by making the radiating surface greater, but could hardly increase the temperature at which condensation sets in, or augment the brilliancy of the condensed material.

How is it with the meteoric theory? The idea in this case is that two meteor streams, the one dense and giving the dark line spectrum approaching us, and the other rarer and giving the bright line spectrum receding from us, have rushed into each other end on, and their collisions have given rise to the light which appears to us as the star. But, if Prof. Vogel's observations are accepted, the spectrum of the star gives us evidence of not two bodies only but of three, moving in different directions and with different speeds. Two meteor streams might indeed run headlong into each other, and exactly in the line of sight, but three could hardly be expected to do so at the same point. Then, if we accept Prof. Vogel's figures, two of these streams are travelling in opposite directions with velocities the sum of which is sixty millions of miles per day, a speed which would have sufficed to carry a body, during the time that the star has been under examination, right across the solar system, from beyond the orbit of Neptune on the one side, to beyond the same orbit on the other. And during the time the spectroscopic observations have been carried on, this speed of travel has been maintained practically unchanged.

The collision of two meteor streams, therefore, is ruled out of court on two grounds. First, we cannot imagine that the Nova is composed of two streams, each moving with a velocity of some thirty millions of miles a day, and each having a length in a straight line equal to the distance of Neptune from the sun, and encountering each other in direct collision, "end on," and both streams lying in the direction of the line of sight. But the second objection is stronger still. The light and heat come from some source, and arrested or retarded motion, or collision—which need not, however, be held to involve the direct impact of the two bodies upon each other—is the source to which the meteoric theory would ascribe them. But there must be the arrest or retardation of motion; the meteoric stream, or star, cannot spend its energy in light and heat, and have its motion as well. And the development of that light or heat cannot precede the loss of speed. The inconceivable rapidity of motion which the relative displacement of the bright and dark lines indicates must be, on this hypothesis, motion *after* retardation; whilst it does not seem in the least likely that one meteor stream should be purely gaseous, and the other give the continuous spectrum, crossed by absorption lines, typical of the incandescent solid surrounded by absorbing vapours.

A variation of this theory suggests one meteor stream plunging into a nebula. This is distinctly an advance, because we could imagine the meteors consumed as they reach and are arrested by the nebula. The bright lines, then, would express the local heating of the gases of the nebula by the friction of the meteorites as they plough their way into the nebula. But we should recognise the bright lines in this case as partly the lines of the elements characteristic of the meteorites, and partly as the typical nebular lines. This is not the case. The typical nebular lines—other than those of hydrogen, which are common to most stars—are all wanting. There is no evidence of a collision between a meteoric swarm and a nebula. The hydrogen lines, the coronal line, many prominent chromospheric lines are recognised, but though one bright line lies close to the chief nebular line the general verdict of the best observers is against any actual identity, and other lines, only less characteristic, are clearly not present.

Yet another circumstance which tells against the theory is, that though the continuous spectrum is over-borne and disguised by the spectrum of bright lines, yet the former is believed to show traces of that peculiar type of spectrum so beautifully shown by Mira Ceti, α Herculis, and other variables. The same circumstance was reported of the first Nova examined by the spectroscope, and the appearance of the violet and ultra-violet hues of hydrogen as bright lines in the spectra of variables like Mira seems to establish an important link between variables of long period, and "temporary" stars like T Coronæ, and our present Nova. But the shaded bands of the third type spectrum are certainly not what we expect from a nebula.

There can be no reasonable doubt, I think, that the basis of our Nova is a real star or sun, and not a mere gaseous nebula, though in some way or other yet unexplained its luminosity was increased for a few weeks one thousand fold. If we suppose a sun, usually quiet and dull, suddenly bursting out with prominences and metallic eruptions on a scale utterly dwarfing anything ever witnessed upon our own sun, we should have a spectrum very like that shown by the Nova, except in one most important detail. We find, in the solar prominences, evidence that the hurling forth of matter with tremendous velocities so heats the cooler hydrogen atmosphere above the chromosphere as to cause it to glow with those ultra-violet lines which are not, at least normally, found in the general spectrum. A speed of 700 miles per second far exceeds anything recorded of the ejective speed of our solar storms, so that we have to conceive of far more violent convulsions than the sun ever displays. But supposing such convulsions brought about, it is easy to imagine that for the time being a far greater part of the light of the star might proceed from what we might call its chromosphere, prominences and corona, than from its photosphere. In such circumstances the bright line spectrum, which would certainly in its principal lines, and probably in its subordinate ones, closely resemble that of our Nova, would be the most prominent feature.

Unfortunately, in this case, the bright lines would be displaced towards the blue instead of towards the red; the heated gases would be approaching us, not receding from us, as we find to be actually the case.

This circumstance is one which leads me to make a suggestion, which I put forward with much diffidence and a clear apprehension of many defects in it, for indeed we are yet far from possessing enough material to theorize upon.

We have seen it happen again and again, in our own solar system, that a comet has swept round the sun at so short a perihelion distance that the two bodies must almost have grazed each other. Suppose we put instead of a comet a long and dense stream of meteors; suppose too that the perihelion distance of the meteor stream lies *within* the radius of the star, and that the orbit of the meteors is so presented to us that their motion at perihelion is almost entirely in the direction of our line of sight. The maximum speed in the orbit which such a stream could attain in our system is 380 miles per second. It is readily imaginable that other suns than ours might be able to raise double such a velocity, that depending upon the mass of the star and the radius of its photosphere. It is quite outside the limits of probability to suppose that two meteor streams should both be moving with a speed of some 350 miles to the second and almost precisely in the line of sight. But there is nothing difficult at all in the supposition that there are many stars which are capable of inducing a velocity of 700 miles per second at perihelion. If then we had a long and dense stream of meteors travelling towards a star, and rushing into it, or just grazing it, as they passed periastron we should have the ordinary spectrum of the star, and superposed upon it the spectrum of the glowing meteorites, and of the components of the stellar atmosphere through which they were rushing. The meteorites would therefore give us a spectrum just as their orbital motion was the highest. This suggestion overcomes the difficulty of supposing that the two colliding bodies are moving both before and after collision with the enormous relative speed which the displacement of the bright lines, as referred to their dark companions, would indicate.

There is another point. A large proportion of the meteors would probably escape capture by the star, and would be seen after periastron on the other side of the stellar disc. But they would be now travelling far more slowly in their orbit, and that orbit would be inclined at a very considerable angle of the line of sight, so that their motion from us would then be at a comparatively slow rate. The principal bright lines, the hydrogen lines certainly, but probably not the lines corresponding to the lower strata of the stellar atmosphere, would then be doubled; the line near the red corresponding to the meteors at periastron, and seen on the one side of the star, and the line nearer the blue corresponding to the meteors after periastron, and seen on the other side of the star. And this is the very appearance which Father Sidgreaves, Prof. Vogel, and other observers have actually recorded.

I should not at all wish to press this suggestion, for the matter is not really ripe for solution. But I should like to point out how vastly more powerful our means of stellar spectrum analysis have become. The present Nova only reached the fifth magnitude, and was entirely overlooked for six weeks after it had reached its maximum. Nevertheless we have acquired stores of spectroscopic information with regard to it which will take long to properly discuss. Its history—especially when the mode of its discovery by an amateur astronomer, armed only with a half-guinea spy-glass, is borne in mind—points therefore to the extreme importance of a diligent watch being kept upon the sky, and especially upon the Milky Way and its off-shoots, for who can tell how many Novæ have flashed out during the past few years which, though not bright enough to attract universal attention, like Tycho's Pilgrim Star of 1572, were yet quite bright enough to have lent a rich harvest of information to the prism and the sensitive plate.

TEMPORARY STARS.

By A. C. Ranyard.

We have to thank Prof. E. C. Pickering for the photographs which illustrate this number of Knowledge. There have been great difficulties in reproducing them, and I must apologize for the unsatisfactory appearance of many of the plates. It has been found very difficult to show the numerous lines and great variations of brightness in the spectrum of the Nova, and at the same time to keep the background of sky in the star plate black. I have preferred that the printing should be done so as to bring out the best effect in the spectrum of the Nova.

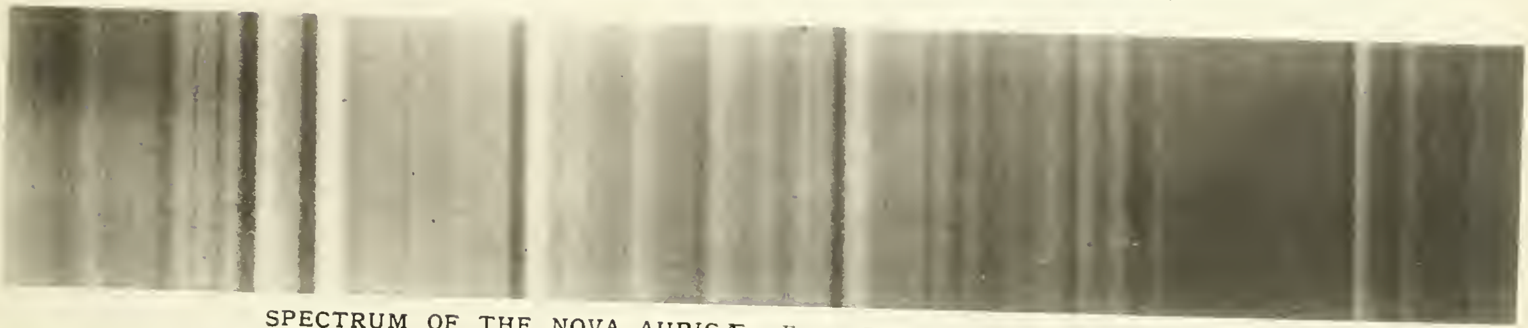
On examining the reproduction of the Nova spectrum given in our plate, it will be noticed that there are at least eight bright lines, bounded in every case by dark lines on their more refrangible sides. This seems to be more than a chance coincidence, and may probably* be taken as indicating that we have before us the superposed spectra of two bodies, one giving bright lines and moving away from us so that all its lines are pushed from their natural places towards the blue end of the spectrum by the apparent shortening of the wave-lengths—very much in the same manner as the note of a steam-whistle is raised as a whistling locomotive approaches us, and depressed as it rushes away from us—and the other body giving dark absorption lines and moving towards us.

The lines F, G, h, H_1, and α are characteristic of the element hydrogen, and, according to Dr. Vogel of Potsdam, who has compared the place of some of these lines with the places of the corresponding lines in the spectrum of stationary hydrogen in a vacuum tube, the hydrogen of the bright-line-giving star is rushing away from us at the extraordinary rate of 230 miles a second, while the hydrogen giving rise to the absorption lines is approaching us with a velocity of 320 miles a second.

The phenomena observed may, it seems to me, be accounted for by the disturbance produced by the motion of a comparatively small star moving away from us through a nebula which was moving towards us. When a large meteor plunges into our atmosphere, the meteor and a considerable mass of gas driven up before it become, for a second or more, intensely luminous. Large meteors frequently leave a trail of luminous gas around their paths, which remains glowing in the cold regions of the upper air for sometimes twenty minutes or half-an-hour. Such trails generally become gradually broader and fainter, and before their final disappearance they not infrequently subtend an angle of half a degree in diameter, as seen from a distance of 70 or 80 miles—that is, they have an actual diameter of about three-quarters of a mile, although the meteoric body whose motion through the air gave rise to the trail may only

* We ought not to speak with certainty until the intervals between the neighbouring bright and dark lines have been more accurately compared. If the interval between the components of different lines is found to be proportional to the wave-lengths of those lines, the phenomenon may be ascribed with much confidence to relative motion; if not, or if the character of the bright and dark lines does not in every case correspond, some other explanation might be preferred. Father Sidgreaves' negatives, which he has kindly lent me for examination, show that there are at least twenty-five pairs of adjacent bright and dark lines, the dark line being uniformly on the more refrangible side; this evidently points to some physical connection.

SPECTRUM OF α LYRÆ. From a Photograph by PROF. E. C. PICKERING.



SPECTRUM OF THE NOVA AURIGÆ. From a Photograph by PROF. E. C. PICKERING.

DIRECT PHOTOGRAPH OF THE NOVA AURIGÆ AND SURROUNDING STARS.

Taken by PROF. E. C. PICKERING.

have been a few inches or a foot in diameter. Thus the energy lost by the meteor becomes distributed as heat through a region considerably larger than that originally disturbed by the passage of the body.

If such a disturbance took place on a stellar scale, we should expect to find the matter of the star mixed with nebulous matter driven into a state of intense gaseous incandescence in front of the moving star, and the material left in its wake probably at first in a state of brilliant gaseous incandescence, but soon condensing into incandescent particles, which would give out a continuous spectrum and form a sort of elongated photosphere, the light of which would be channelled by the absorption due to the cool surrounding nebulous matter. We should thus have a bright-line spectrum in which the position of the lines corresponds with the velocity of the moving star, and an absorption spectrum corresponding with the velocity of the absorbing nebulous matter.

The lines which we find as bright in the spectrum of the Nova correspond with lines which are the last to glow in the solar chromosphere, as the matter shot up in the prominences cools.* They, therefore, correspond with the lines which would be the first to glow on being heated.

It is evident that the nebulous matter must be very sparsely distributed, for the velocity of the star does not seem to have appreciably altered in the three weeks, during its passage through the densest part of the nebula. In this period, it must have passed across a region more than equal to the diameter of the orbit of Jupiter. During the whole period in which the star, according to Prof. Pickering's observations, remained above the eleventh magnitude, it must have passed across a nebulous region of greater diameter than the orbit of Uranus. If the matter of the solar system were distributed uniformly through a sphere of the same diameter as the orbit of Uranus, we can calculate the mass of the matter which would be cut through by the passage of a body as large as the earth diametrically across such a nebula; and though no doubt a much larger mass of the nebula would be displaced and heated, it seems probable that a body of the same mass as the earth would not lose an appreciable portion of its velocity in giving an equal velocity to the quantity of matter in the first instance disturbed.

A very significant fact as to the distribution of these temporary stars is that they have all appeared in and about the region of the Milky Way, which is a region of extensive nebulæ. One of the most recently observed of these temporary stars appeared actually near to the centre of the great Andromeda nebula, and its spectrum did not appreciably differ from the spectrum of the nebula. If it exhibited bright lines, they were, like the bright lines of the Andromeda nebula spectrum, exceedingly faint.

THE FLOWER OF MAHOMET.

By the Rev. Alex. S. Wilson, M.A., B.Sc.

The Prophet-plant (*Arnebia echioides*) is a native of Persia and Arabia, but has been introduced and grows freely in gardens in this country. Its chief interest lies in its variable flowers, which may fairly rank with those of the changeable Hibiscus and other

> "Plants divine and strange
> That every hour their blossoms change."

The plant is about two feet in height, and somewhat resembles a cowslip or an auricula. It belongs to the natural order Boraginaceæ, and is nearly allied to the lungwort, viper's-bugloss, borage and forget-me-not, all of which exhibit colour-changes more or less distinct. The various species of Myosotis or forget-me-not are also called scorpion grasses, from the upper flower-bearing portion of the stem being curled on itself like a watch-spring. The cluster of flowers, forming the inflorescence of Arnebia, develops in the same scorpioid fashion. There is a double row of flower-buds on the curled stalk, and as this gradually unwinds pair after pair of the flowers expand in succession. In shape and colour the individual flowers are not unlike those of the primrose, though rather smaller. When a flower first opens five conspicuous jet black spots are seen upon the yellow rim of the salver-shaped corolla. If the flower be examined the following day, we are surprised to discover that the black spots have vanished as if by magic. The yellow of the corolla is also much paler, and a little later on presents quite a bleached or silvery appearance, the petals becoming almost white. No sooner have the spots disappeared from the first pair of flowers than a second pair expand, and display their sable marks in bold relief upon the yellow enamel of their petals. From this time onwards the inflorescence comprises both kinds of flower, those but newly opened having the five conspicuous spots, and older ones on which no spots are visible. From these dark spots—the so-called finger-marks of Mahomet, Arnebia has received its name—the Prophet-plant. Its flowers seem bewitched, the change is so pronounced and obvious; a day or two after unfolding they differ so much from the newly-opened ones beside them, that were they growing on separate plants, we should at once set them down as belonging to another species.

This change of colour gives rise to another interesting peculiarity. If Arnebia be examined by daylight, and again in the dim twilight, the observer is struck by a remarkable circumstance. In broad daylight, the golden spotted flowers at once arrest the eye, while their paler companions are hardly observed. The inflorescence owes by far the greater part of its display to the younger flowers. In the dusk this is entirely reversed; the conspicuousness of the inflorescence now depends on the paler flowers, and the others are so obscured that a second glance is needed before they can be discerned. The relative brilliancy of the two sets of flowers can also be tested by gradually retiring from the plant, keeping the eyes still fixed on the blossoms. At dusk the young flowers are lost sight of much sooner than the others; by day the older ones first disappear in the distance. This peculiar transformation imparts to the inflorescence of Arnebia a faint similitude of the pillar of cloud by day and of fire by night—that celestial manifestation of sacred story so closely associated with the native region of this desert flower.

Here, then, we have one of those phenomena which, for the naturalist, possess all the fascination of a mystery. What can be the explanation of this remarkable change of colour, and what advantage does the flower derive from the sudden disappearance of its spots and the blanching of its petals?

With the reader's permission, we shall now proceed to show why Nature has bestowed on Arnebia what she has denied to the leopard—the power of changing its spots. Before we can say why any flower should change its colour, we must first know why a flower is coloured at all, and why all flowers are not coloured alike. Almost all the peculiarities of flowers can be explained as having reference to the visits of insects. The honey is secreted

* The vertical storms continually taking place upon the sun preclude the idea that the upper chromosphere is a region only occupied by hydrogen and 1474 stuff. The gaseous matter of the sun must be completely mixed by diffusion as well as churned by solar storms. But in the outer and cooler regions certain elements continue to glow when others have become non-luminous.

as an inducement, while the scent and brilliant colours serve to attract the attention of the honey-gatherers. The researches of the late Charles Darwin demonstrated the importance of cross-fertilization in the vegetable kingdom. Very many flowers are quite sterile with their own pollen; in other cases, although the flower has the capacity of self-fertilization, the resulting seeds are of very inferior quality compared with those obtained as the result of cross-fertilization. As carriers of pollen, then, insects perform an essential service to plants, and it is in order to secure their services that flowers are brightly coloured.

For the variety of colour observed among flowers there appear to be two principal reasons. A little reflection will show that, since flowers are so dependent on insects for the conveyance of their pollen, it must be to the advantage of each species of plant to possess flowers distinctively coloured and capable of being easily recognised by honey-seeking insects. A bee does not visit all flowers indiscriminately; it would be greatly to the flowers' disadvantage if it did. In the course of a single journey the bee for the most part restricts itself to the flowers of one species, and has been known to visit as many as thirty dead-nettles in succession, passing over all other flowers. Time is saved by this method, for by keeping to one kind of flower at a time the insect becomes familiar with its outs and ins, and the practice thus acquired enables it to overtake a larger number of blossoms than it could if it did not observe this rule. This constancy in visiting the same kind of flower is of great importance to plants, since it insures that the pollen will be conveyed to a flower of the same species as that from which it came. But if all flowers were coloured and perfumed alike, the winged botanist could not identify the species; the pollen would be constantly transferred to the stigmas of the wrong flowers, where it would be useless, and so the work of cross-fertilization would be seriously impeded.

A second cause contributing to the variety observed among flowers is the desirability of attracting special kinds of insects. As we have just seen, an insect does not visit all kinds of flowers indiscriminately; neither, on the other hand, does a flower attract indiscriminately all kinds of insects. Not only are injurious and unprofitable visitors excluded, but the more specialized insects are in greatest demand. Partiality for particular insects is shown both by the shapes and colouring of flowers. Open shallow flowers, with exposed honey accessible to almost all insects, have, as their most frequent visitors, short-lipped flies and beetles. Many blossoms, again, have become specially adapted to bees. Their honey is placed beyond the reach of short-lipped flies, and requires the slender proboscis of a bee or butterfly for its extraction. Honeysuckle, Habenaria, Plumbago, Phlox, and Narcissus illustrate a third type, with flower-tubes so narrow and deep that their nectar is quite inaccessible even to bees, and is reserved entirely for moths and butterflies, which possess an extremely long and thin proboscis. There is a corresponding adaptation in the colours; the gay tints of the buttercup, poppy, and rose appear to have special attractions for beetles; bees show a decided preference for blue, and this colour predominates in flowers whose shapes are adapted to their visits. Deep tubular flowers specialized for Lepidoptera, fall into two divisions, according as they solicit the attentions of diurnal butterflies or nocturnal moths. Red and purple are the favourite colours of the former, while nocturnal moths show a preference for white and pale flowers. Thus, the carnation and campion (*Lychnis diurna*) which open by day, have dark tints in comparison with *Lychnis vespertina*, which unfolds its petals towards evening. Almost scentless by day, this white nocturnal flower diffuses a delicious fragrance in the twilight. The evening primrose (Œnothera), which however, has yellow petals, is another example of this class. But the most remarkable plant of this type is the night-flowering stock (Cereus). Its pale blossoms open about seven in the evening, emit puffs of odour from time to time, and close up again towards midnight; by morning the flowers are withered. It is impossible to doubt that we have in this instance a flower specialized for the visits of nocturnal moths. The reason why nocturnal flowers, like the honeysuckle and evening campion, have pale-coloured petals, is not far to seek. These pale hues can be much more easily distinguished at night than the red and purple of Dianthus or Githago. Among lilies, both diurnal and nocturnal flowers occur, and clearly indicate by their colours to which section of the Lepidoptera they are adapted. The Turk's-cap lily, with its perianth of fiery scarlet, is a characteristic example of a diurnal flower adapted to butterflies which wander abroad in day-time. On the other hand, Lilium Martagon and L. candidum with their white bells are nocturnal lilies fertilized by night-loving moths.

Two flowers, unlike in their colouring, can hardly be equally attractive to the same visitors, even if they grow together on the same plant, as is the case in Arnebia; the presumption, therefore, is that its spotted and pale blossoms are adapted for different insects. Moreover, the stronger colours of the younger flowers correspond with those of the day-blooming class, while the paler tints of those in the second stage will render them more attractive to nocturnal moths; and this view is strongly confirmed by the fact that night-blooming flowers are never variegated, but have their petals uniformly devoid of markings. By night the dark spots tend, in this instance, to conceal the blossoms so much, that, if these are to be converted into nocturnal flowers, the removal of the spots is absolutely necessary. We may therefore conclude with tolerable certainty that the flowers of Arnebia in their first stage are adapted to bees and diurnal Lepidoptera, while in their second condition they array themselves in paler hues to attract nocturnal moths. By the colour-change, in this instance, a diurnal is converted into a nocturnal flower, and one advantage thereby gained is that the blossoms appeal to a larger class of fertilizing agents. The more restricted the circle of visitors on which any plant depends the greater the risk, in the event of insects being scarce, of its flowers remaining unfertilized and perishing. Here it would seem that Nature proceeds on the same principle as a fisherman in changing his bait. Like some other variable blossoms, Arnebia is in the advantageous position of carrying two strings to her bow.

(To be continued.)

Letters.

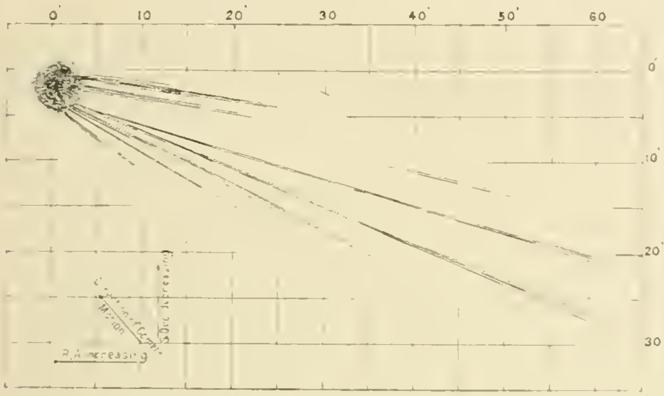
[The Editor does not hold himself responsible for the opinions or statements of correspondents.]

SWIFT'S COMET.

To the Editor of Knowledge.

Dear Sir,—We were unable, owing to cloudy weather, to see Swift's comet until the morning of March 11th; it was then very hazy, and clouds were passing. With the large telescope, only a faint elongation on one side indicated a tail; no sign of rays or other peculiarities could be seen. The star camera was turned to it and a photograph taken with 110 minutes' exposure. When developed, this showed

a tail composed mainly of five ribbon-like rays, the longest of which measured 35′ in length. The rays were equidistant; probably more would have appeared, but the bright moonlight produced a slight general fog on the plate. Cloudy, wet weather followed, and we did not see the comet until the morning of March 22nd. Even then clouds were passing, but the air was clearer. A plate was exposed in the star camera, and out of 2h. 30m. we got 1h. 55m. effective exposure; this photograph shows more of the old rays, one being out to the edge of the plate 70′ long, and three new ones not seen before. One of the first five was visible at a distance from the head, but it could not be seen joining the coma. All these details could be easily seen with a suitable light, but they are too faint to reproduce photographically; and the plate used having on it a grating in squares of 5′, it was decided to make a careful drawing on an enlarged scale, a process rendered very easy



Drawing from negative of Swift's Comet.

by the grating on the negative. It was made by Mr. Sellars, and faithfully represented what could be seen on the negative, excepting only the brilliance of the rays, which it was found necessary to make a little brighter in order to get them to photograph. The negative shows a projection of luminous matter to the sun, and its gradual turning back to form two of these rays. This and other features have been reproduced in the drawing, and the grating on the negative has made it possible to follow these details. Two of the rays could be seen to the margin of the plate, and probably extended beyond it.

Every one is familiar now with the selective action of the sensitive plate in regard to the light of coloured stars, and also in that of nebulous light, for, in photographs of nebulæ, we find details of light and shade, and a structure shown which is wholly invisible through the telescope. In this comet photograph, the same action has evidently brought to light ribbon-like rays which would otherwise have remained unknown; for in the part of the tail visible through the telescope no sign of rays could be detected, even when I knew from the photograph that they were there, and were photographically brighter than the other parts. They are a photographic feature then, and not a visual one in this comet's tail; or, to put it another way, we have a comet with coloured rays in its tail, probably blue or violet rays from the preponderance of blue in cometary spectre. And the sensitive plate, by its *selective powers* as well as by its power of storing up faint light rays, is an aid to vision, and may be used to make a preliminary analysis of cometary or nebulous light, and point the way for more searching analysis by the spectroscope. One morning, through a break in the clouds, we photographed the comet with fifteen minutes' exposure and got rays showing faintly; at the same time, atmospheric conditions were such that we should have photographed an eleventh magnitude star in six minutes. H. C. RUSSELL.

Sydney Observatory, 16th April, 1892.

[The evidence afforded by Dr. Russell's photographs of the bluish tint of certain regions in the tail of this comet is very interesting. It does not necessarily follow that the bluish tint was caused by gaseous incandescence within the streaks, of a different character from that existing in other parts of the comet's tail, or that structures within comets' tails will ever be detected with the spectroscope, for the spectroscope only enables us to detect images due to bright line incandescence and monochromatic light. The bluer character of the light dispersed by these narrow regions may be (and probably is) due to the finer grain of the particles which disperse the sun's light in the regions comprised within the streaks. It has now been known for some years that the light dispersed by the tails of comets is partially polarized in a plane passing through the sun, indicating that the greater part of the light is dispersed by particles which are small in diameter compared with the wave-length of light. Some twenty years ago Lord Rayleigh showed that the colour of the light dispersed by fine particles depends on the fineness of the particles,* and that the finer the dispersing dust the richer is the scattered light in short wave-lengths or blue rays. Thus the dust from Krakatoa at first caused the sun to appear red. As the larger particles floating in the air fell to the ground the sun became green, and ultimately it became blue.—A. C. RANYARD.]

THE GLACIAL PERIOD AND THE PLANET MARS.

To the Editor of KNOWLEDGE.

DEAR SIR,—It seems to me that one of the chief secondary causes of the Glacial Period has not heretofore been sufficiently enforced. During the short winters and long summers, whatever precipitation occurs will be largely in the form of rain. On the other hand, during the long winters and short summers it will be, on the whole, chiefly in the form of snow. Now the snow, by its great reflective power, will cause the earth to lose a very large proportion, perhaps nearly three-quarters, of whatever radiant energy does fall upon it. Moreover, during the short summer, when the sun is able to melt the snow, there will be an extensive evaporation from its whole surface, forming clouds. These will in their turn reflect away the sun's rays, and at the same time by their shade protect the snow beneath them from melting.

This cause requires for its action the presence of considerable moisture upon the surface of the planet. It has been stated that the glacial theory does not appear to apply to Mars. In the case of this planet, however, we have good reason for thinking that it formerly had extensive oceans upon its surface, and the gradual cooling to which it has been subjected has enabled room to be formed for them in its interior; that is to say, there are probably extensive regions in its interior which are not sufficiently hot to convert water into steam. The water would therefore, naturally, as is the case with the earth, go underground, filling all the microscopic cavities between the rocks. Be that as it may, the appearance of the surface of this planet leads us to think that it possesses at present very much less water in proportion than does our earth. That being the case, there could not be sufficient evaporation to form the extensive snow-caps required by the glacial theory. As an illustration of this point, we should

* See a paper "On the Light of the Sky, its Polarization and its Colour," by the Hon. J. W. Strutt (now Lord Rayleigh), in the *Phil. Mag.* for February, 1871, pp. 107-120, and March, 1871, pp. 274-279.

expect that the northern or continental slopes of the Himalaya Mountains would be colder but also drier than the southern ones, which are exposed to the ocean. Yet upon the warm southern slopes we find the line of perpetual snow considerably lower than upon the colder northern ones. Thus, because the supposed snow-caps upon Mars are small, it does not necessarily indicate that the temperature of the planet is higher than that of the earth.

William H. Pickering.

Arequipa, Peru, April 14th, 1892.

[Prof. W. H. Pickering's interesting suggestion, that as a planet cools its oceans will be absorbed and go to augment the volume of its underground waters, does not commend itself to my mind as explaining the phenomena we observe on Mars. For, in order that the earth should absorb say a mile in depth of its ocean covering, the isothermal surface within the earth, at which water is converted into steam, would need to fall at least three or four miles, and all other isothermal surfaces would fall with it; that is, the mean temperature of the surface, even in the equatorial regions, would certainly fall far below the freezing point. But this does not seem to be the case on Mars.

If we assume, as most speculative astronomers have hitherto assumed, that the white polar caps of Mars are due to snow, it follows that the mean temperature of the Martian surface in the equatorial and temperate regions of the planet must be above 32° Fahr., a fact which is not consistent with the assumption so frequently made, that the mean temperature of a planetary surface must vary inversely as the square of the distance of the planet from the sun. We are, it seems to me, forced to assume either that the polar caps of Mars are not due to snow, or that the mean temperature of the equatorial and temperate regions of Mars is above 32° Fahr.; that is, that they are much warmer than they should be on the assumption that the mean temperature will vary inversely as the square of the distance from the sun. To my mind, the discrepancy is most easily accounted for by supposing that the atmosphere of Mars is more dense than the atmosphere of the earth. I do not see any necessity for the assumption that the amount of the atmosphere and the amount of the ocean on Mars must bear the same proportion to the amount of solid material as on the earth, or even that they must have originally borne the same proportion. It is, of course, possible that we may be observing phenomena on Mars quite dissimilar to those going on here. The white polar caps may be due to the deposition of the snow-like crystals of carbonic acid, which evaporates again at a temperature far below the greatest cold which we experience on the earth's surface.—A. C. Ranyard.]

A CORRECTION.

To the Editor of Knowledge.

Dear Sir,—My attention has been called to an error in the *derivation* of the expression for the tangential force in the article on the "Origin of Binary Stars," and I gladly seize the earliest opportunity for correcting the mistake. The height of the tide obviously varies as the *first power* of the tide-generating force, *not as the square*, as asserted near the bottom of the first column on page 82. The couple acting against the rotation of Helios arises from the excess of the attraction of Sol on the nearer tidal protuberance above that on the further. Now this excess is found to vary inversely as the third power of the distance between the two bodies. But the couple also varies directly as the height of the protuberance (*i.e.*, as the height of the tide), and this height varies inversely as the third power of the distance. Hence the tidal frictional couple varies as the inverse sixth power of the distance; or it may be described as varying inversely as the square of the tide-generating force, since the tide-generating force varies as the inverse cube of the distance. If we denote the tidal frictional couple by T, the radius vector by ρ, and the tangential force by t, the principle of action and reaction gives for the equilibrium of the forces $t\rho = T$, or $t = \frac{T}{\rho} = \frac{\kappa}{\rho^7}$. In the fifth line from the top of page 82 (second column), the reading should be "cube" instead of "sixth power." Since the result reached in the article was correct, the error in the *derivation* of the expression for the tangential force does not vitiate my argument, and the conclusions are therefore sound. But as a friend had kindly called my attention to the slip, I thought it ought to be corrected.

Very faithfully yours,

Berlin, May 20th, 1892. T. J. J. See.

CAN A PLANET BE FINALLY SURFACED WITHOUT A SEDIMENTARY ROCK SERIES?

To the Editor of Knowledge.

Dear Sir,—Professor Judd, in his instructive work on Volcanoes, at page 305, tells us that "the Moon appears to be destitute of atmosphere and water . . . under the circumstances we find its surface, as we might expect, to be composed of rocks which appear to be entirely of igneous origin"; and at page 367 he refers to the striking evidence, on the Moon, of the action of volcanic forces, in the vast size of the so-called "craters" (up to 50 and 60 miles diameter).

But inasmuch as Professor Judd's entire work is a peculiarly good demonstration that "without water, there can be no volcano," I should esteem it a great favour if any of your scientific readers can explain the obvious contradiction in the above.

For over ten years I have carefully studied the lunar surface, in the hope of eventually solving this well-known astronomical riddle, and have come to the conclusion that such a globe cannot have been finally surfaced (solely) by igneous and volcanic agency.

The convection and radiation of heat from rock surfaces, as the initial temperature declined, must have been such an exceedingly slow process that it is inconceivable how a globe of that size can have passed from a semi-molten to the airless and waterless stage of development, without the intervention of a very prolonged era of erosive denudation.

Selenographers are agreed that on our moon there is practically an absence of river valleys, and the fluviatile sculpturing so characteristic of our earth's surface; in fact, that the lunar surfacing seems to be due entirely to a deposition of solid dry material, showing a singular and marked absence of all drainage phenomena.

Hence it is urged that there is an absence of sedimentary stratified rocks, what we see being the unchanged results of primeval lava lakes, volcanic outbursts, and "cinder rings," laid during an era of such high initial temperature that water, near the surface, would have been a physical impossibility.

The subsequent decline in temperature, again, is assumed to have been so exceedingly rapid as to preclude the possibility of any erosion of river valleys, and the formation of sedimentary rocks.

An idea prevails that the enormous craters and volcanoes (seen even all over the poles) originated during the period of high initial temperature, and that the cavities, 20 and

50 miles in diameter, were in some way blown out explosively.

What I desire to point out is the improbability—not to say the impossibility—of this being done by volcanic action, as we understand it nowadays, and at a time when the lunar crust, to the very surface, must have been far above the boiling point of water. "Without water there can be no volcano," and hence, with such a high temperature, there being no *water*, there could have been no volcano. There is no escape from this syllogism.

But another weighty argument against the lunar craters being due to volcanic action is their vast size. This is usually accounted for by the fact that lunar gravitation is but one-sixth of ours, thereby enabling an "explosion" to eject matter to six times the distance. When we look into the case, however, we notice that these lunar "safety valves" are only loaded with one-sixth the terrestrial weights. The explosive force, or accumulated ejecting power (due to steam held under superincumbent pressure), is thus reduced in the same ratio, and the power of ejecting matter on the earth and moon is the *same*. The power not being derived from any explosive material, such as dynamite, but being controlled by gravitation, the fallacy of the old reasoning is obvious.

Again, by all accounts our moon at one time rotated on its axis more rapidly, and has been slowed down by vast tides.

If these were of *water*, it is clear that the globe had (then) cooled down so far as to be practically rigid, and the tidal deformation (as in our case) was confined to the fluid envelope, which thus needs must have scoured off all the "cinder rings," and completely filled in the craters and sunk plains, large and small, the ellipsoidal figure of the globe, so strongly insisted on by Proctor (as a true cause for libration), arising subsequently.

If, however, the era of tidal retardation occurred while the globe was in the semi-molten and plastic condition, as Sir R. S. Ball suggests, then the formation of the walled plains (lava lakes) and volcanic craters must have taken place later on, yet still in the absence of water, inasmuch as there is no evidence of its presence then nor yet afterwards, when, if anything, it should have been still more obvious; when one would very naturally expect the enormous atmosphere of vapours to fall by condensation and scour the surface persistently for long ages *during the slow decline in temperature*, at last, perhaps, leading to a development of polar caps and general glaciation.

There is abundant and beautiful evidence in the surfacing of the absence of water, during its formation, and this very evidence, I hold, is fatal to any theory of volcanic surfacing, for which water is an essential element. Even the poles are covered by vast lava lakes, "cinder rings," and so-called volcanoes.

This singular absence of all traces of water, all sculpturing by rivers, and of drainage phenomena in the lunar surfacing, is the great problem which needs solution: whether an era of erosion with deposition of stratified rocks and formation of river valleys is, or is not, logically a *necessary sequel* to a semi-molten stage in planetary evolution; whether a planet could pass from the molten to the airless and waterless stage, and yet retain all through the later stages the surfacing due to a primeval igneous era.

To me it seems easiest to assume that the moon has long ago passed through our terrestrial erosive stage, and now, in its airless and waterless condition, is swathed from pole to pole in snow and ice formations.

Sibsagar, Assam, 15th March, 1892. S. E. Peal.

[While agreeing with Mr. Peal that the traces of water action upon the moon are not conspicuous, I do not feel sure that there is no evidence of sculpturing by rivers or drainage phenomena. Mr. Neison, in his book on "The Moon," p. 73, is inclined to think that in many points the rills or clefts "bear some resemblance to the dried beds of lunar watercourses or rivers. Thus many of these rills commence at the end of a system of branched valleys leading from a highland, whilst others can be detected winding along the bottom of extensive valley regions." It seems to me also that many of the dark delta-shaped patches, which are frequently found in the planes round lunar mountains, afford evidence of drainage phenomena. There are two such patches on either side of the lunar Appennines, which are well shown on photograph No. 1, published with the number of Knowledge for December, 1890, and three such dark patches to the west of Copernicus in photograph No. 2 in the same number. These dark regions are also well shown in the photograph published in the May number for 1890.

Though no doubt the greater number of terrestrial volcanoes are adjacent to the sea, some, as those between Siberia and Thibet, and in the Chinese province of Manchouria, as well as the extinct volcanoes of central France, are at a considerable distance from the sea or lakes. Mr. Scrope long ago pointed out that though a considerable amount of steam escapes from active volcanoes, it does not follow that the water producing it was originally derived from the sea. All rocks contain a considerable amount of water of crystallization, and we are probably not warranted in saying that a volcanic irruption could not take place without the presence of an adjacent sea, lake, or river.

Though I do not think that the albedo of the lunar surface has been determined with the accuracy which Zöllner supposed, it seems to me that we have evidence that the whiter portions of the lunar surface are very white compared with any terrestrial rocks, and that it is more probable that the lunar mountains are capped with snow than that the higher regions are formed of very white rocks, while the valleys and low-lying regions are always formed of much darker material. I therefore agree with Mr. Peal as to a large portion of the lunar surface being covered with ice or snow, but it is easier to me to account for the great ring formations, as being analogues to terrestrial volcanoes, than to suppose that they are rings of ice and glacial phenomena of which we have no terrestrial analogues.—A. C. Ranyard.]

THE CORONA OF THE SUN AND STARS.

To the Editor of Knowledge.

Dear Sir,—Although the teachings of modern astronomy have led us to look upon many of the fixed stars as veritable suns, merely reduced by their vast distance to a subordinate lustre, the possibility of being able to observe the corona about stars seems to have escaped notice.

During the last seven years, the principal work of this observatory has been the observation of a number of long period variable stars, which decrease in lustre from about the 5th to below the 13th magnitude, in periods ranging from 200 to 600 days. They differ in a remarkable manner from the ordinary fixed stars. Most of them are of a deep red or ruddy colour, and many are more or less nebulous; they may be divided into four classes, viz., stars having

(*a*) A remarkably well-defined, almost planetary, disc;

(*b*) Well-defined stars surrounded by a more or less dense, ruddy atmosphere;

(c) Large, woolly stars, with ill-defined image, resembling a small but bright planetary nebula.

(d) Stars which at minimum show, in place of the variable, a slight bluish nebulosity.

In the case of a star surrounded by a very faint nebulosity or coronal appendage, we should not expect to see any trace of the corona till the star was nearly or totally obscured, and recent observations show that in several instances, when the star has gradually become so faint as to be invisible with our $6\frac{1}{10}$ inch refractor, its place has been occupied by a pale bluish nebulosity, which has again vanished as soon as the star reappeared, being evidently so faint as to be readily overpowered by a small amount of stellar light.

It may be that all variables are nebulous, or have extensive and bright coronas, but that when the nebulosity is faint, we only become aware of its existence when the light of the variable is reduced to a minimum.

Yours faithfully,

Rousdon Observatory, Lyme. C. Grover.

THE DEPTHS OF THE MEDITERRANEAN AND BLACK SEAS.

By Richard Beynon, F.R.G.S.

ISOLATED as it is from the great water masses of the globe, the Mediterranean—with its off-set, the Black Sea—may be regarded as a provincial sea. The oceanic circulation of the North Atlantic sweeps past its narrow entrance unheeded. The great tidal wave is effectually debarred by the convergence of the African and European coasts from influencing the tidal phenomena of the Mediterranean, and the same cause, aided by the near approach of the strata underlying the Straits of Gibraltar to the surface, precludes the possibility of the chill waters that ever roll equatorwards along the sea floor finding their way into the vast inland sea under discussion. The geographical limits of the Mediterranean are well known, but its true geological boundaries by no means coincide with these. Instead of terminating to the westward at the Straits of Gibraltar, the sea is really continued some 50 miles into the Atlantic, for the shoal water which separates the line of coast between Tangier and Ceuta from the opposite shores of Spain extends westwards to that distance. Here the shallow ridge terminates, and the sea bed rapidly falls into the depths of the Atlantic.

The proximity of the island of Sicily to Cape Bon suggests the very natural division of the Mediterranean into an eastern and western section. Taking the western portion, we find that at its two lateral extremities it is separated by a shallow ridge from the Atlantic on the one side, and the deep waters of the Eastern Mediterranean on the other. The depths of water obtaining on these shallows approximately coincide. The deepest sounding obtainable on the ridge between Cape Bon and Sicily is under 200 fathoms, while the maximum depth in the vicinity of Gibraltar is 180 fathoms.

The shoal water which commences some 50 miles to the westward of Gibraltar is really continued 120 miles to the eastward of Point Europa, almost to the shores of Alboran Island.

If we regard as continental, islands that are separated from the mainland by depths not exceeding 100 fathoms, then Alboran must be classed as an oceanic island, for on all sides it is surrounded with water of 400 fathoms and upwards in depth. The Balearic group present most peculiar features. Instead of forming one group, as their juxtaposition would seem to imply, they make two. Iviza and Formentara are separated from Majorca, Minorca, and the Spanish coast by soundings of 300 fathoms. The two last named islands have a channel of 50 fathoms between them, and to the eastward of Minorca the sea bed has a steep gradient until, 30 miles from the Balearic group, a depth of 1400 fathoms is encountered. A comparatively slight upheaval of the Mediterranean bed would suffice to connect Corsica and Sardinia, for the Straits of Bonifacio are of little depth. Shoal water, too, connects this group with Elba and the mainland of Italy. The shallow channel which extends from Cape Corso, *viâ* Elba, to the coast of Tuscany has an average width of from 15 to 20 miles, and nowhere along it can soundings of a greater depth than 50 fathoms be obtained.

We now come to the easterly boundary of the western portion of the Mediterranean Sea.

From Cape Passaro, at the south-easterly corner of Sicily, a bank with 300 fathoms of water over it extends to the opposite shores of Tripoli, while a somewhat similar ridge, with a lesser depth of 200 fathoms, connects the other extremity of the island with Cape Bon. Between these two banks a deep water gully runs, with an average depth of 600 to 700 fathoms.

With regard to the deep water areas of the western section of the Mediterranean, a fairly uniform depth, ranging between 1200 and 1600 fathoms, is maintained between Marseilles and Algiers, while the deep water lane extending from Naples to Sardinia admits of soundings of 1500 to 2000 fathoms.

These are the more salient features revealed by soundings taken in the western portion of the Mediterranean. Scientific research, however, has added much to our knowledge of the eastern section during the past few years, and it is chiefly to Austria that progress in the study of the oceanography of this part of the Mediterranean is due.

Before the *Pola* expedition the generally received greatest depths obtained in the Mediterranean were 2040 fathoms in the western section and 2150 in the eastern. The latest results, however, show that deeper soundings are obtainable. On the 28th July, 1891, the *Pola* found the depth of 2406 fathoms, and a few miles further to the eastward 2236 fathoms, both of which depths exceed those mentioned above. The exact position of this, the deepest spot yet discovered, is 35° 44′ 20″ north lat., and 21° 44′ 50″ east long., or, roughly speaking, about 50 nautical miles south-west of Matapan. Very properly the Austrian Hydrographical Board have determined to perpetuate the record of their nautical find by assigning to this deep-water spot the name of Pola Deep. This discovery will necessitate the removal of the deepest part of the Mediterranean considerably eastwards from its present position on our maps. Another deep-water area explored by the *Pola* was that lying between Candia and Alexandria, the depths ranging from 1810 fathoms, some 20 miles south-east of Grandes Bay, to 1322 fathoms within a short distance of Alexandria. The serial temperatures taken by this expedition coincide in the main with those obtained during previous researches.

From 80·8° F. to 69° F. was the thermometric range in the first 27 fathoms. In the next 27 fathoms the temperature fell to 62·5° F. The range for depths between 110 and 547 fathoms was 59° F. to 57° F. At the lowest depth found (2406 fathoms), the temperature was 56° F., which, as previous investigators have established, is the approximate uniform temperature of the bed of the Mediterranean. One very curious result of the temperature experiments was the finding of water whose temperature was $52\frac{1}{2}$° F., at a depth of 415 fathoms, at the junction of

the Adriatic with the main waters of the Mediterranean Sea.

It has been found that the temperature of the Mediterranean Sea bed is by no means constant, and, according to some authorities, varies slightly in accordance with the mean temperature of the winter preceding the season in which the temperatures of the sea bottom are taken. Thus in 1871 the *Shearwater* expedition, under Captain Nares and Dr. Carpenter, found a bottom temperature at 1650 fathoms of 56°, and the year previous the same temperature had been met with at a spot where the sea bed was 1743 fathoms from the surface. In 1881, however, Captain Magnaghi, Hydrographer to the Italian Navy, along with Professor Giglioli, in the surveying vessel *Washington*, found the bottom temperature to be 1° higher than that recorded as the mean of those obtained in 1871. The mean temperature of the months of December, January, February, March, and April is 53·6° F. at Toulon and 56·84° F. at Algiers, and the average of these two temperatures gives approximately the degree of heat contained in the Mediterranean Sea bed between those two places.

With regard to the Adriatic Sea, soundings show that only one-third of its area can be regarded as forming a part of the Mediterranean basin proper, the remaining portion not averaging more than 50 fathoms in depth. A channel of 400 fathoms stretches across the entrance to the sea, from Otranto to Albania. Within the sea the depth increases until a maximum of 765 fathoms is attained, and this rapidly shoals until the comparatively shallow waters of the northern portion of the sea are encountered. The *Pola* made some interesting experiments relative to the transparency of the Mediterranean waters. In three cases a white disc was seen down to a depth of 177 feet. Where the water was deepest, however, invisibility was reached at 105 feet.

The paucity of animal life in the great depths of the Mediterranean is well known. Its depths are to a certain extent stagnant. There is an utter absence of that vertical circulation so thoroughly developed in the Atlantic, and which results in process of time in every particle of water being alternately transposed from sea bed to surface, and surface to sea floor. The only semblance of such a circulation that exists in the Mediterranean is caused by the descent of water that has been concentrated by evaporation on the surface, and has thus had its specific gravity raised above that of the underlying strata. But the descent of this water will be seriously interfered with at a depth of 200 or 300 fathoms, where the temperature is such that it will encounter an aqueous layer whose specific gravity is much akin to its own.

It will be remembered that it was owing to the absence of life met with during the researches of Professor E. Forbes, in the Ægean Sea, that the erroneous doctrine was formulated that marine oceanic life ceased at a depth of about 300 fathoms.

Subsequent explorations in the deep sea speedily showed the fallacious character of such a conclusion, except in enclosed seas of the Mediterranean type.

In the western basin of the Mediterranean, the bottom consists chiefly of clay, of a grey or brownish colour. It always contains some carbonate of lime, the remains of foraminifera. Both in appearance and chemical constitution the mud resembles that dredged up in the open ocean from areas which are shut off by submarine ridges from free participation in the vertical oceanic circulation.

In the eastern section of the Mediterranean the sea bed deposits contain a considerable proportion of volcanic ash and other constituents of igneous origin.

Before proceeding to discuss the character of the connecting channels and currents that unite the Mediterranean Sea with the Atlantic Ocean and the Black Sea, we will briefly allude to the findings of the latest researches conducted in the waters of the last named sea.

The Russian gun-boat the *Tchernomoretz* was engaged in June and July, 1890, in the work of surveying. The maximum depth, 7365 feet, was found in the central portion of the sea, between the Crimea and Anatolia. The explorations were continued last year, and the results of the previous year's work were confirmed. The 100 fathoms line was found to lie close to the shores of the Crimea and Anatolia, and the axis of greatest depression has a direction from south-west to north-east. The steepest coast was found at Rizo, where the angle of inclination attains 10°. The most interesting of the recorded observations are those relating to the temperature of the Black Sea waters. The variations of temperature at the surface range from 77° F. to 41° F., while on the northern shores the thermometer sometimes falls below the freezing point. The annual variations of temperature, due to the seasons, do not penetrate deeper than 100 fathoms. At a depth of from 30 to 175 feet the temperature was 57° towards the south coast, 54° in the centre, and 52° near the east, west, and north shores.

The water begins to be warmed by the air in the month of May, and during August the mean temperature of the surface water is higher than that of the superincumbent air. The variation of temperature for depths below 180 feet is very peculiar. At this point the thermometer registers 45° F. Then the thermometer begins to rise, and at a depth of 6000 feet it shows 49° F. For all depths below 200 fathoms the temperature may be described as constant, and lying between 49° F. and 48° F. The most distinctive feature of the Black Sea, however, is that at the depth of 450 feet distinct traces of sulphuretted hydrogen occur. The quantity increases with the depth, until at 600 feet it is quite sensible, and at the mean depth of 940 feet it renders animal life quite impossible. Some even place the inferior limit of organic life at so high a level as 100 fathoms. Dredgings show that at one period of geological history the Black Sea contained an abundance of low organisms, and the semi-fossil shells of certain molluscs characteristic of the brackish water of the lagoons of the Caspian and Black Seas are much *en évidence*. These fossils are doubtless the remains of the Pontic fauna of the Pliocene period, when the Black Sea basin was not connected with the Mediterranean. The salinity of the Black Sea was then by no means so great as it is now. When the connection between the two seas was made, the water from the Mediterranean would make its way as it does at present into the Black Sea area, and speedily lead to the disappearance of the ancient fauna. Thus the sulphuretted hydrogen is one of the products formed by the decomposition of the older life, and as the water in the great depths is practically stagnant, *i.e.*, quite motionless, it follows that the decay is an exceedingly slow process.

Assuming that the water which annually flows through the Bosphorus into the Black Sea forms a one-thousandth part of the total contents of the sea, it will take 1000 years to completely renew the whole contents of the basin. It will be thus easily seen to what small extent the deep waters participate in the scheme of circulation. The Sea of Azov is merely the expanded mouth of the River Don, its waters being shallow, having no greater depth than $7\frac{1}{2}$ fathoms, and being thoroughly mixed by each storm that visits it.

The Sea of Azov, too, shares in the disturbing influences of the surface current which sweeps round the shores of

the Crimea to the north-west, and then follows the trend of western shores past the mouths of the Danube towards the Bosphorus. The dimensions and velocity of this current are augmented when the melting of the snow in the Black Sea basin is more rapid than usual.

We have mentioned above that a decided influx of Mediterranean water takes place into the Black Sea. Were it not for this saline water the Black Sea would be much fresher than it is, and were the connection between it and the Mediterranean destroyed then the sea would become fresher, as there is a surplus of river and rain supply over evaporation. Throughout the whole length of the Bosphorus, the Sea of Marmora, and the Hellespont, two distinct currents can be traced, the heavier Mediterranean water forming the underlying stratum, moving slowly *into* the Black Sea, the lighter water from the latter sea being superincumbent and moving in the opposite direction. There appears to be very little mixing of the two currents, the layer of demarcation between the two being easily detected by the difference in the specific gravities of the two aqueous masses.

The comparatively fresh water that the Black Sea contributes to the waters of the Mediterranean produces but little effect, so large is the area of the basin into which it pours itself and so small relatively is the volume of water so contributed.

The greatest depth of the Sea of Marmora is found along the line connecting the Hellespont with the Bosphorus, and ranges between 266 and 355 fathoms.

The Hellespont itself has a depth of 50 fathoms, while the mean depth of the Bosphorus varies from 30 to 40 fathoms.

The Ægean Sea has not its specific gravity reduced as might be expected below that of the whole Mediterranean by the influx of the Black Sea water. In fact its specific gravity is greater than that of any other section of the Mediterranean basin.

The accompanying table bears out the truth of this. The figures quoted are the result of many observations, and are derived from samples of water taken from 50 miles to the westward of Gibraltar to the easternmost section of the Black Sea.

Mediterranean Water outside Gibraltar	1·0260	—	1·0270
Mean specific gravity for Western Section	1·0280	—	1·0290
Mean specific gravity for Eastern Section	1·0290	—	1·0300
Mean specific gravity for Black Sea	1·0120	—	1·0140

In each case the results are derived from analyses of surface water.

In round numbers the area of the Mediterranean basin is one million square miles, and the average rain-fall over the whole area that drains into it has been assessed as being equivalent to an annual rain-fall of 30 inches upon the sea itself. The amount of water removed by evaporation is greatly in excess of this, probably $2\frac{1}{2}$ times as great. At Rome the evaporation is represented at some 105 inches per annum, at Madrid it is 65 inches, and at Cairo 92 inches. It does not require much speculation to determine what would happen if the waters of the Mediterranean were not replenished from some external source. A shrinkage of the basin by a diminution of the water area would continue until the evaporation from the reduced surface would equal the amount of aqueous precipitation. But before that condition would be reached, the shrinkage would have resulted in the drying of the bank between Sicily and Africa, and between Africa and Gibraltar, with the result that two "dead" seas would be formed.

Happily, there is not much probability of such a change taking place, for the Mediterranean is supplied with water from the Atlantic as well as the Black Sea. Through the Straits of Gibraltar there flow two currents, as there do through the Sea of Marmora. The existence of an outward current has been long known. It is accountable for the higher temperature found in the deep waters of the Eastern Atlantic. Water, unmistakably of Mediterranean origin, has been found some 200 miles north-west of the Straits at a depth of 1560 fathoms. Its presence at such a depth is readily understood when it is remembered that the water of the Mediterranean contains an average of 3·9 to 4 per cent. of solid matter in a state of solution, while the percentage in Atlantic water in the vicinity of the Mediterranean entrance is 3·4 to 3·5.

It has been calculated that the *inflow* through the Straits is equivalent to a river eight miles wide, 100 fathoms deep, running with an uniform velocity of $18\frac{1}{2}$ miles in the twenty-four hours. Such are the dimensions of a current requisite to maintain equilibrium between the contribution of rivers, precipitation supply, and the inflow from the Black Sea on the one hand, and the copious evaporation and the outflow into the Black Sea and the Atlantic Ocean on the other.

THE FACE OF THE SKY FOR JUNE.

By HERBERT SADLER, F.R.A.S.

AT the time of writing these lines several fine spots and groups are visible on the solar surface. Throughout June there is no real night, but either daylight or twilight. A minimum of the Algol-type variable U Coronæ will occur at 10h. 11m. P.M. on the 20th.

Mercury is technically a morning star during the first portion of the month, but is in reality too near the Sun to be observed. He comes into superior conjunction on the 20th. Venus, during the greater portion of the month, is *the* conspicuous object in the evening sky. She attains her greatest brightness on the 4th, when she is more than three times brighter than she was on January 1st. She sets on June 1st at 11h. 14m. P.M., or 3h. 9m. after the Sun, with a northern declination of 24° 16′, and an apparent diameter of 36″, $\frac{29}{100}$ths of the disc being illuminated. On the 9th she sets at 10h. 45m. P.M., or 2h. 34m. after the Sun, with a northern declination of 22° 49′, and an apparent diameter of 40·8″, $\frac{21}{100}$ths of the disc being illuminated. The crescent form of the planet may now be easily seen in a good opera or field glass, if Venus is viewed before sunset or just after, so that her glare does not interfere with distinct vision. On the 17th she sets at 10h. 10m. P.M., or 1h. 53m. after the Sun, with a northern declination of 21° 18′, and an apparent diameter of $46\frac{1}{2}$″, about $\frac{13}{100}$ths of the disc being illuminated. On the 24th she sets at 9h. 29m. P.M., or 1h. 10m. after the Sun, with an apparent diameter of $51\frac{1}{2}$″, $\frac{7}{100}$ths only of the disc being illuminated, while her brightness has fallen to one-half of what it was on June 4th. After this date she is too near the Sun to be clearly seen. During the month she hovers on the confines of Gemini and Cancer; but does not approach any bright fixed star.

Mars is an evening star, but owing to his great southern declination is badly placed for observation in these latitudes. He sets on the 1st at 0h. 3m. A.M., with a southern declination of 20° 23′, and an apparent diameter of $14\frac{3}{4}$″, the phase on the south-western limb amounting to 1·5″, and

the light of the planet amounting to rather less than one-third of what it will be at the beginning of August. On the 17th he rises at 11h. 14m. P.M., with a southern declination of 20° 4′, and an apparent diameter of $17\frac{1}{2}''$, the phase amounting to $1\frac{1}{3}''$. On the 30th he rises at 10h. 34m. P.M., or 2h. 16m. after sunset, with a southern declination of 20° 26′, and an apparent diameter of 20″, the phase amounting to 1″, and the brightness to about six-tenths of what it will be at its maximum. During the month Mars describes a short direct path in Capricornus, but does not approach any bright star very closely. As Jupiter does not rise till after midnight on the last day of June, we defer an ephemeris of him till next month.

Saturn is an evening star, and is still well situated for observation. He rises on the 1st at 0h. 28m. P.M., with a northern declination of 4° 48′, and an apparent equatorial diameter of $17\frac{1}{2}''$ (the major axis of the ring system being $40\frac{1}{2}''$ in diameter, and the minor 0·3″). On the 30th he sets at 11h. 31m. P.M., with a northern declination of 4° 18′, and an apparent equatorial diameter of $16\frac{3}{4}''$ (the major axis of the ring system being 38·6″ in diameter, and the minor 0·7″). During the first portion of the month the ring system is invisible in small telescopes. The following phenomena of the satellites may be observed (the times are given to the nearest quarter of an hour). June 5th, $10\frac{1}{4}$h. P.M., Tethys, eclipse reappearance; June 8th, $11\frac{1}{2}$h. P.M., Dione, eclipse reappearance; June 19th, $10\frac{1}{4}$h. P.M., Dione, eclipse reappearance; June 22nd, 10h. P.M., Tethys, eclipse reappearance; June 30th, 9h. P.M., Dione, eclipse reappearance. During the month Saturn describes a short direct path in Virgo, but does not approach any naked-eye star. He is in quadrature with the Sun on the 14th.

Uranus is well situated for the purposes of the amateur observer, rising as he does at 4h. 16m. P.M. on the 1st, with a southern declination of 11° 54′, and an apparent diameter of 3·7″. On the last day of the month he rises at 2h. 19m. P.M., with a southern declination of 11° 43′. During the month he describes a short retrograde path to the N.N.W. of λ Virginis. A map of the path of Uranus is given in the *English Mechanic* for February 12th. Neptune is invisible.

There are no very well marked showers of shooting stars in June.

The Moon enters her first quarter at 9h. 51m. A.M. on the 2nd; is full at 1h. 32m. P.M. on the 10th; enters her last quarter at 9h. 1m. P.M. on the 17th; and is new at 2h. $6\frac{1}{2}$m. P.M. on the 24th. She is in apogee at 6·6h. P.M. on the 5th (distance from the earth 251,690 miles); and in perigee at 2·4h. P.M. on the 21st (distance from the earth 227,250 miles). Her greatest eastern libration occurs at 4h. 40m. A.M. on the 14th, and her greatest western at 3h. 54m. P.M. on the 22nd.

Chess Column.

By C. D. Locock, B.A.Oxon.

All communications for this column should be addressed to the "Chess Editor, *Knowledge Office*," and posted before the 10th of each month.

The April problem is unsolvable. The composer's intention 1. R (R8) to R6, discovered by Alpha and H. S. Brandreth, is defeated by 1 . . . P × R, as pointed out by Alpha.

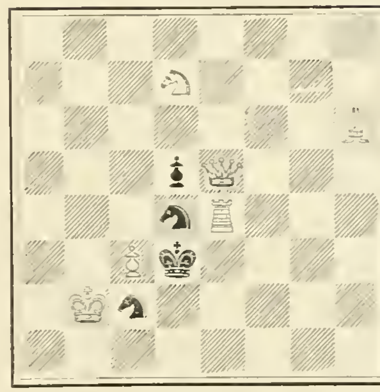
C. T. Blanshard.—The emended version enclosed is open to the same objection as the other.

C. J. O.—If 1. R to Q8ch., K × P, and there is no mate.

PROBLEM.

By C. D. Locock.

Black.



White.

White to play, and mate in two moves.

MORE CHESS FALLACIES.

(*Continued from March*, 1891.)

VI. *That the majority of Pawns on the Queen's side is advantageous.*—In the first place there is nothing magical in the Queen's side; if both players have castled on the Queen's side, then the advantage lies with the player possessing the majority of Pawns on the *King's* side. If, on the other hand, White has castled on the Queen's side and Black on the other side, Black having most Pawns on the King's side, and White on the other wing, then White has a slight advantage; for the White King at QB square can reach K3 in two moves, while the Black King at KKt square must take three moves to reach Q3. Hence the corollary that it is better for end-game purposes to castle on the Queen's wing. The King is not only more central, but almost certainly has an exit available at Q2 towards the centre.

VII. *That an early sortie of the Queen is inadvisable.*—On an open board, especially after one or two exchanges, the sooner the Queen comes out the better. If there is one strong move more habitually overlooked than any other by good players, it is probably the move *Q to KKt4*. Owing to the fact that the adverse QB and KKt generally guard against the move, the opportunities for it are necessarily of rare occurrence: the result being that when they do occur they are neglected.

VIII. *That Bishops of opposite colours always tend to a draw.*—The contrary is often the case. Imagine that Black has castled on the King's side and weakened his position by the move P to KKt3. It is now to White's advantage that each player should lose his King's Bishop, leaving the Bishops of opposite colours. The White Queen's Bishop in conjunction with the Queen and a Knight or Rook will then probably be irresistible on the King's side, White's Bishop commanding the holes at KB6 and KR6, while the Black Queen's Bishop is practically useless for defence. The general rule may be stated as follows: If your opponent has all, or nearly all, his Pawns on one colour, get rid, if possible, of his Bishop of the *other colour*. The remaining Bishop may guard some of the Pawns, but he cannot guard what is more important—the *diagonals among the Pawns*

The following is the fourth game of a match for the Championship of America now in progress. It is understood that Mr. Steinitz does not claim the title.

[Vienna Opening.]

White (Lipschütz).	Black (Showalter).
1. P to K4	1. P to K4
2. Kt to QB3	2. Kt to KB3
3. P to KKt3	3. B to K2 (a)
4. B to Kt2	4. Kt to B3
5. KKt to K2	5. P to Q3
6. Castles	6. B to Q2 (b)
7. P to Q3	7. Q to Bsq
8. B to KKt5	8. P to KR3 (c)
9. B × Kt	9. B × B
10. Kt to Q5	10. B to Qsq (d)
11. P to KB4	11. B to Kt5
12. P to B3	12. Kt to K2
13. Kt to K3	13. B × Kt
14. Q × B	14. P to QB3
40 min.	55 min.
15. P × P	15. P × P
16. Q to R5	16. Kt to Kt3 (e)
17. Kt to B5	17. Castles.
18. Q to Kt4 (f)	18. K to R2
19. R to B3	19. P to B3 (g)
20. P to KR4	20. R to KKtsq (h)
21. P to R5	21. Kt to Bsq (i)
22. B to R3	22. Q to B2 (j)
23. Kt to R4	23. Q to Kt3ch
24. R to B2	24. Q to K6
25. K to Kt2 (k)	25. Q × QP (l)
26. Kt to Kt6!	26. Q to B5
27. Q to B5	27. Resigns (m)

Notes.

(a) A very tame reply to a not very formidable attack. A much more enterprising line of play would be 3. . . B to B4 ; 4. B to Kt2, P to QR3 ! (to preserve the Bishop), followed by Kt to B3, P to Q3, and the moment White castles P to KR4 ! The present annotator has adopted this counter-attack with success in more than one match-game.

(b) Again feeble. The Bishop should go to Kt5. If then 7. P to KB3, the Bishop retires to Q2, followed by P to KR4 soon ; or if 7. P to KR3, B to K3, to be followed by Q to Q2 and P to KR4. White castled too soon.

(c) Waste of time. If he meant anything by his last move he should consistently play B to R6.

(d) This endeavour to preserve his two Bishops cramps his game terribly. He might play Kt to K2. If White take the Bishop the doubled Pawn can always be got rid of, and there is the open file for what it is worth.

(e) Clearly if 16. . . Q to K3, the reply 17. B to R3 wins a Pawn.

(f) To prevent Kt to K2, as he does not wish his Knight disturbed, and threatening also P to KR4.

(g) There seems to be nothing better. The position now is accidentally a good illustration of some remarks above on Bishops of opposite colours. The Black Bishop being of the same colour as the Pawns on the King's side is at a disadvantage, while the White Bishop has the advantage of being able to occupy the diagonals among these Pawns. The Bishops of opposite colours *prevent* the game being drawn.

(h) This and the next move leave his King and Rook shut in. Perhaps R to B2 would be better.

(i) Intending to fix the Knight afterwards at Kt4 if permitted. Kt to K2 seems much superior. White's reply threatens Kt × RP.

(j) He has not time for this diversion. 22. . . Kt to K3 is out of the question on account of 23. Q. to R6ch and 24. Kt × RP. His best course seems to be either 22. . . B to Kt3ch and 23. . . Q to Ksq, or 22. . . P to KKt3.

(k) Tempting the Queen to go to Kt4, whence she would have no escape ; partly too on account of the threatened B to Kt3.

(l) If 25. . . Q to Kt4, 26. Q to K2, P to KKt3 ; 27. P × P, followed by B to B5 or R to B5, according to whether Black retake or not. Perhaps his best resource lay in 25. . . B to Kt3 and 26. . . R to Ksq, with a view to Kt to K3, but the game seems lost anyhow.

(m) For the Bishop at last will get on the diagonal with fatal effect ; *vide* Note (g).

CHESS INTELLIGENCE.

The Scotch Championship has again fallen to Mr. D. Y. Mills, who scored 9½ games out of 11 ; Mr. G. E. Barbier was second, one point behind the winner.

The Committee of the World's Fair are arranging for an International Tournament at Chicago next year.

It is stated that another telegraphic match will be played this summer between Steinitz and Tschigorin—this time, probably, without restrictions as to the openings. During the course of the match, Dr. Tarrasch and M. Tschigorin will engage in a match over the board, either at Berlin or St. Petersburg. A year or two ago, M. Tschigorin's chance of success would have been considered slight, but he has had plenty of first-class practice lately, and should at any rate make a good fight against the German champion, whose opportunities for match practice are limited.

Another Divan Handicap was in progress last month. The leading scores are at present, Loman, 10 ; Lee, 9 ; and Van Vliet, 7½.

Messrs. Blackburne and Lasker commenced a match for £50 a side, at the British Chess Club, on May 23rd.

The match goes to the winner of the first six games, draws not counting. Play takes place every week day, except Wednesdays, beginning each day at 2 p.m. The time limit is 18 moves an hour.

The following is the score in the Lipschütz—Showalter match in America : Lipschütz, 4 ; Showalter, 1 ; drawn, 4.

Contents of No. 79.

TERMS OF SUBSCRIPTION.

"Knowledge" as a Monthly Magazine cannot be registered as a Newspaper for transmission abroad. The Terms of Subscription per annum are therefore as follows:—To any address in the United Kingdom, the Continent, Canada, United States, Egypt, India, and other places in the Postal Union, the Subscription is 6 shillings, including postage ; or 1 dollar 50 cents ; or 6 marks ; or 7 francs 50 cents.

For all places outside the Postal Union, 5 shillings in addition to the postage.

Communications for the Editor and Books for Review should be addressed, Editor "Knowledge" Office, 326, High Holborn, W.C.

KNOWLEDGE
AN ILLUSTRATED
MAGAZINE OF SCIENCE
SIMPLY WORDED—EXACTLY DESCRIBED

LONDON: JULY 1, 1892.

CONTENTS.

PROTECTIVE RESEMBLANCE IN ANIMALS.

By R. Lydekker, B.A.Cantab.

THAT the colours of animals tend to assimilate themselves to the natural surroundings of the animals themselves is a fact which has been long known in natural history; and it is, indeed, one which is self-apparent to every sportsman and to every traveller in the wilder regions of the globe. For instance, everyone is probably aware that desert-haunting animals, like lions, gazelles, wild asses, jerboas, and many species of birds, generally have a uniform sandy-coloured coat, which renders them at a short distance almost or completely invisible in their native wastes. Then, again, every English sportsman knows how completely the coloration of the partridge and the hare assimilates with that of the stubble or ploughed fields in which they are wont to lie; while the mottled blacks and browns of the woodcock and snipe accord so exactly with the hues of decaying leaves and grass that the inexperienced eye will often fail to detect a wounded bird even when lying close to the feet, and scarcely anyone can distinguish the living birds when on the ground. The brilliant vertical orange and black stripes of the tiger and zebra, when seen in a menagerie or a museum, do not strike us as resembling anything in inanimate nature. In its native jungle, largely composed of upright yellow stems of tall grasses, between which are narrow intervals of deep black shade, the colour of the tiger is, however, admirably suited to its surroundings: and it is stated that the stripes of the zebra are arranged in such proportions as exactly to match the pale hue of arid ground by moonlight, so that on such occasions these animals are absolutely invisible even at very short distances. We hardly need refer to the white colour of polar animals, such as bears, ermines, foxes, hares, &c., as the most perfect example of this kind of protective coloration; and numerous other examples will at once present themselves to the reader.

Well known as are these comparatively simple instances of protective resemblances, there are, however, others of a more striking nature, where the animal either resembles the form of some inanimate object, or that of some other kind of animal which has especial means of protection; and since these resemblances are less generally known, they will form the subject of the present article. The term "protective resemblance" is generally applied to those instances where the animal resembles more or less closely an inanimate object, and thus renders itself inconspicuous; while the instances where one animal assumes the appearance of another, and thereby becomes conspicuous, are classed under the term "mimicry." It will, however, be obvious that both these kinds of resemblances are near akin, and are far in advance of protective coloration, pure and simple, where no imitation of form takes place. We shall first mention some instances of the imitation of the forms of inanimate objects by animals, and then refer to those cases where other animals are the objects of imitation.

Some of the best examples of what we shall take leave to call inanimate mimicry are to be found among insects, and we shall take our first case from among the butterflies. All are probably aware that a large number of these insects, such as our common peacock and tortoiseshell butterflies, while brilliantly coloured on the upper surfaces of their wings, have the under surfaces of the wings of a dull, sombre hue; and most of us have doubtless been almost startled at the suddenness with which one of these gaudy creatures seems to vanish altogether when it settles on dark ground or the rough bark of a tree, and at once closes its wings. Here, then, we have an instance of ordinary protective coloration, without any attempts of mimicry of form. There is, however, a peculiar group of butterflies allied to our own purple emperor, inhabiting Northern India and the Malayan region, which have gone far beyond this simple kind of protective resemblance, and actually imitate the form of leaves growing on their native branches. These butterflies (scientifically known as *Callima*), one of which is figured in the accompanying cut, have the upper surface of the wings brilliantly marked with orange, their front wings terminating in a sharp point externally, and the hind ones in a "tail," after the fashion of our swallow-tailed butterflies. Between the sharp point of the front and the tail of the hind wing there runs on the under surface a curved line, from which smaller lines are given off to the edges of the wings. When this butterfly settles on the stem of a plant bearing pointed leaves and closes its wings, the points and tails of the same of course come into exact opposition; and since the tail of the wings is closely applied to the stem of the plant it appears exactly as though it were the stalk of a leaf, the midrib and veins of which are exactly imitated by the lines on the under surface of the wings; while the apex of the leaf is formed by the opposed points of the front wings. So exact is the resemblance of the butterfly when in this position to a faded leaf, that, as Mr. Wallace tells us, it deceives the eye even when gazing full upon it;

The Leaf-Butterfly. (After Wallace.) *

and without actually seeing the insect settle upon the spot it is absolutely impossible to find it. To increase the delusion, no two individuals of these insects are precisely alike on the under surface ; while many of them have little black patches, or dots, exactly resembling the dark fungous growths so often found on decaying leaves. Fortunately for the reader who desires to verify this extraordinary instance of inanimate mimicry, a case is now exhibited in the central hall of the Natural History Museum with several of these insects attached to a bough with faded leaves, and it is curious to watch the visitors to this case and see how often they fail to distinguish all the butterflies from the leaves which they imitate.

The next best instance of inanimate mimicry among insects occurs in the so-called stick- and leaf-insects, which are allied to our grass-hoppers and cockroaches. The stick-insects, of which some are found in Southern Europe, have long, slender bodies and limbs, of a dark colour, and so exactly resemble dry sticks that it is almost impossible to distinguish between the one and the other. To increase the resemblance, these insects when at rest have the habit of placing their legs unsymmetrically. On the other hand, the leaf-insects, or "walking leaves," of India, both in colour and form, so exactly simulate green leaves that they may be passed dozens of times without attracting attention. All the legs of these curious creatures are furnished with irregular flat expansions looking precisely like bitten leaves ; while the head and fore part of the body forms a kind of stalk expanding behind into a broad and flattened abdomen, covered by the horny wings which are veined and netted so as to form an almost exact imitation of a leaf.

We might cite many other instances of inanimate mimicry among insects, but we must pass on to show that this phenomenon is by no means confined to this group of animals. Perhaps we should scarcely expect to find this kind of mimicry in such a comparatively highly organized a creature as a fish ; yet there is a group of fishes, familiar to those who have kept aquaria, under the name of sea-horses, in which it is exhibited in its full perfection. The ordinary sea-horse attaches itself to a sea-weed or some other object by curling its tail tightly round it ; and all these fishes have the habit of anchoring themselves by their tails in some way or another. In all of them the hard, horny body is furnished with a number of prominent ridges and spines; but in one, a peculiar group from the Australian seas, these spines attain an enormous development, many of them being prolonged into irregular filaments or streamers of skin, which are especially developed throughout the long and slender tail. As these streamers float in the water they so exactly resemble both in colour and shape the particular kind of sea-weed to which these fishes are in the habit of attaching themselves, that the whole creature seems but part and parcel of the fucus ; so that when on the sea-bottom it must be impossible for any carnivorous rover to distinguish between the animal and the vegetable.

One more instance of this kind of mimicry, and we must close this portion of our subject. This example is taken from the mammalian, or highest class of animals, and, although not such a perfect imitation of form as those we have already mentioned, is very remarkable as occurring so high up in the animal kingdom. Most of our readers are probably acquainted, at least by name, with those lowly South American mammals known as sloths. These animals are inhabitants of the great forest regions of that continent, and are of a sombre greyish colour, very like that of the gnarled and lichen-clad boughs, from beneath which they are wont to hang back-downwards. Not only, however, is their general colour like that of a lichen-covered branch, but their coarse grey hairs actually develop a growth of lichens upon themselves to complete the resemblance to their surroundings. It is, indeed, clear that the long grey coat of the sloths has been produced for the sole purpose of this protective mimicry, for when this is removed there is found beneath an under coat of softer fur marked by yellow and black stripes, which may be pretty confidently regarded as the original coloration of these animals.

We have now to consider animate, or true mimicry, in which one animal imitates the form, and generally the habits, of another in order to participate in the immunity from foes enjoyed by the latter, owing either to the possession of some formidable weapon, or to its unpalatable nature as food. In all cases of this kind of mimicry it is essential that the mimicked animal should be numerically far more abundant than the mimicker, as otherwise predatory creatures would soon learn that the inocuous and palatable animal was more likely to be captured than the harmful one. Undoubted cases of true mimicry are most common among insects, and it is to these alone that our observations will be confined. We may also observe that mimicking insects, as a rule, mimic other insects, although it has been considered that some large caterpillars mimic snakes, and certain moths certainly imitate birds.

We will first refer to some excellent and well-marked instances of mimicry which occur among the insects of our own country. Most of us are probably familiar with those large

* We are indebted to Messrs. Macmillan & Co. for this figure.

hairy brown flies which may be seen in autumn creeping in a sleepy sort of manner about the windows of houses, and are commonly known as drone-flies, and scientifically as *Eristalis*. These insects, although true flies, with only a single pair of wings, are so like bees (in which, it need scarcely be said, there are two pairs of wings) that it is very difficult to persuade some persons that they are not really members of that group of insects. Their resemblance to the latter is increased by their similar habits, more especially their bee-like buzz; and there is no doubt whatever but that they are mistaken by birds for bees, and thereby enjoy an immunity not granted to ordinary flies.

Again, the gaudy flies marked with bold bands of black and yellow which are so common on fine summer days in gardens, and are known as wasp-flies (*Syrphus*), take their name from their resemblance to wasps, which in some species is so close as to make it difficult to convince people that they are not really wasps.

Less common than the above-mentioned flies are the beautiful British insects known as clear-winged hawk-moths. Some of these, named hornet clear-wings (*Sphecia*), so exactly resemble large wasps or hornets that they would deceive nine persons out of ten who are not entomologists. Moreover, they have precisely the same habits as hornets, and when caught will actually curl up their bodies in a wasp-like manner as if about to sting, although they are perfectly harmless. Less complete is the resemblance of other clear-wings—hence known as bee clear-wings—to humble-bees. These insects, as has been well observed, are, however, very important, as proving that their mimicry is an acquired character, since when they first emerge from the chrysalis their wings are thinly covered with the well-known minute scales characteristic of ordinary moths, these scales soon falling off and leaving the wings perfectly transparent. This indicates that the ancestors of the clear-wings had wings like other moths.

In all the foregoing instances the mimicking insects imitate various members of the Hymenopterous order; but we have now to notice a case where a moth imitates a bird so completely as to deceive even the best observers when the two creatures are on the wing together. The moths in which this kind of mimicry occurs take their name of humming-bird hawk-moths from this very circumstance, and are represented by a species not very uncommon in some parts of our own country. So close is the resemblance between these moths and humming-birds that Mr. Bates tells us that, when on the Amazons, he has actually shot specimens of the former in mistake for the latter; and the natives of these regions are firmly convinced that both are of the same species. The extended proboscis of the moth does duty for the slender beak of the bird, while the end of the body of the former is expanded into a kind of brush which imitates the tail of the bird. Humming-birds are, of course, not seized as prey by insectivorous birds, and hence the moths escape their natural enemies from their resemblance to the humming-birds. In our own country, where there are no humming-birds, it is somewhat difficult to see what advantage its bird-like form is to the humming-bird hawk-moth, and possibly its comparative rarity may be due to the absence of the birds it mimics.

We come now to those very remarkable cases of mimicry, as exemplified among the butterflies, where one species mimics one or even more members of the same order, owing to the immunity of the latter from the attacks of birds on account of their unpalatable taste. That the mimicked butterflies are protected by their unpleasant taste has been amply proved by their being offered over and over again to birds, by whom they are as invariably rejected. Their immunity from attack is further proved by their slow flight, and by the bright colouring of the under sides of their wings, so that they have no means of concealing themselves. Most of these mimicked butterflies are found in tropical and sub-tropical regions, and belong to the great families known as *Danaidæ* and *Heliconidæ*. In America these butterflies are usually mimicked by various species of the family of "whites" (*Pieridæ*), which, as we all know in the case of our common cabbage butterfly, are eagerly sought by birds; and the difference of the mimicking species from an ordinary white by the assumption of the bright colours of the Danaids is so great that nobody but an entomologist would imagine for a moment that it even belonged to the same family. It is, moreover, curious that there is one instance where two species of Heliconids inhabiting adjacent regions are respectively mimicked by two varieties of one and the same species of "white."

Stranger even than this, however, is the case of certain South African swallow-tailed butterflies. In this group, as a rule, both sexes are alike, and furnished with the characteristic "tails"; but in one South African species the females entirely lose their appendages, and alter their coloration and the form of their wings so as to mimic not only one, but actually three distinct species of Danaids. Here, then, we have an instance in which a single species of butterfly exists under four totally distinct forms; viz., the typical swallow-tailed male, and the three varieties of tailless females respectively mimicking the three Danaids. No one would have the faintest idea that the three females belonged to the same family, let alone to the same genus and species, as the male; while the three varieties of the female would be assigned without hesitation to as many distinct species. That female butterflies are more often protected by mimicry than the males is a fact which may probably be explained by their extreme importance to the race, and also from the circumstance that when heavily laden with eggs they are more likely to fall a prey to birds than are the lighter males.

A great deal more might be said on the subject of mimicry in butterflies, but we must pass on to our last instance of this feature, which is, perhaps, the most peculiar of all. In this case the mimicked insect belongs to that peculiar group of ants which have the curious habit of carrying in their mouth a leaf which extends backwards over their bodies, and apparently acts as a kind of shade. Now, in British Guiana, there is an insect allied to the cicadas and other bugs, in which the leaf borne by these ants is represented by the thin and laterally flattened body of the creature, which is so compressed that it does not exceed a leaf in thickness, while its jagged upper border simulates well enough the irregular contour of the leaf carried by the ants, of which the borders are generally gnawed by the bearers. Although the legs and lower part of the body of this most curious insect are reddish in colour, the leaf-like upper part of the body has assumed a green hue exactly resembling that of the ant-borne leaves. In a drove of cooshie ants, as the leaf-bearers are called, the mimicking insect is distinguishable solely by its somewhat inferior size; this difference is not, however, sufficiently great to attract the attention of birds, which have learnt by experience that the cooshies are by no means palatable morsels.

It would be beyond the scope of the present article to enter upon the difficult question of the means whereby these mimetic resemblances, whether to animate or inanimate objects, have been produced; but sufficient has been said to show that an amount of interest lies in the

subject, and those whose interest has thus been aroused may perhaps have the good fortune to discover new and unsuspected instances of one or other of these types of protective resemblances.

SOME PRACTICAL APPLICATIONS OF ELECTRICITY.

By J. J. Stewart.

(*Continued from page 226, Vol. XIV.*)

IV.—Incandescence Lamps.

There are two important types of lamp used in illumination by means of electricity—that of arc lamps, described shortly in my last paper, and that in which the immediate source of light takes the form of a thread of carbon raised to a white heat. In describing this second form of lamp it may lead to clearness to consider first, as an example, that special kind of incandescence lamp known as Swan's; for the difference between the various adaptations of glowing carbon to lighting purposes is one of detail merely, the main principle being the same in all.

Mr. Swan tells us that for long it had seemed to him that if ever the electric light was to become generally useful it would be through some application of the incandescence of carbon; and he made a long series of careful experiments in his endeavour to hit upon some method whereby incandescent carbon might be rendered an illuminating agent which would last and give a permanent light.

It is now thirty years since Mr. Swan performed the following experiment in his investigation of this subject. He caused a number of strips of card and paper to be surrounded by charcoal and then put in a crucible, which was afterwards raised to a white heat in a pottery kiln. These pieces of cardboard were in the form of an arch half an inch in length and a quarter of an inch across. They were changed to strips of carbon by the intense heat of the furnace, and their ends were fixed in square carbon blocks by means of small clamps. When these carbon threads, thus prepared, were placed in glass vessels and the air exhausted by means of an air pump, Mr. Swan was delighted to see the threads brighten up with a ruddy glow whenever an electric current from a voltaic battery of 40 or 50 cells was sent through them, and he felt confident that all that was needed was a stronger current in order that they should give out a brilliant white light. Swan believed that this was the first occasion on which carbonized paper was made use of in the construction of an incandescence electric lamp. At that time (somewhere about the year 1860) the cheapest source of the electric current was the voltaic battery, but, as in the case of arc lamps, the introduction of the dynamo machine with its powerful currents gave a great impetus to the attempt to utilize glowing carbon.

After Swan's earliest experiments the matter rested, so far as he was concerned, for seventeen years, till about the year 1877, when he was again led to take up the subject. Not only was there a difficulty at first in getting a strong current at small cost, but the limits attainable in producing a high vacuum were nothing like what they became after the introduction of the Sprengel air pump. The durability of the incandescent filament of carbon depends very much on the perfection of the vacuum obtained in its containing globe; because in the presence of the oxygen of the residual air the carbon consumes away more or less rapidly. Crookes had shown, in his beautiful experiments with the radiometer, how good a vacuum could be attained by means of the Sprengel pump, and Mr. Swan adopted this method of producing a high vacuum in his glass globes. In conjunction with Mr. Stearn, who had had a large experience in the use of the Sprengel pump, he mounted some of his carbonized paper filaments in the globes provided for them, and then exhausted the globes of air as completely as possible. As showing the importance of attention to minute details, it is interesting to note that in this case it was found that the carbon absorbed some of the air around it while it was cold, and then gave out this air when it was heated strongly by the passage of the current, thus partially destroying the carefully prepared vacuum. The device of heating the carbon threads strongly by passing through them a current *while the process of exhaustion was going on* was made use of, and thus the air entangled in the carbon was driven out before the globes were finally sealed. It is by careful attention to such minute particulars as these, which less earnest investigators might have overlooked, that the present brilliant success in electric lighting has been reached. The wires leading the current to the lamp are in connection with supporting clips, and great care has to be used in order to make a good contact between the threads of carbon and the metallic clip, and such an one as shall be able to resist the high temperature to which the junction is exposed. In Swan's lamps the carbons were thickened at their ends, and, in some of the first trials, electrotyping and hard soldering of the ends of the carbon to platinum was applied.

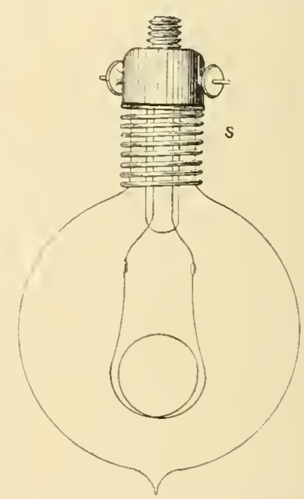


Fig. 1.—Shows the Swan Lamp connected up and ready for use. The spiral spring *s* (shown apart from the lamp in Fig. 2) is compressed round the neck of the globe, and holds it with a firm yet flexible grip, the electrical contact being perfect, and the whole connection leaving little to be desired in point of neatness and efficiency.

In 1878 Edison took up the subject of incandescence electric lighting. After unsuccessfully attempting to make a durable conductor out of a mixture composed of infusible earth with carbon and metallic substances, he proceeded to use platinum, still without success. Afterwards he went on, with his characteristic unflagging energy, to try "carbon wires," as he discovered that a thread of ordinary sewing cotton, when properly carbonized, remained unbroken for a long time, even at extremely high temperatures, when it was placed in a bulb exhausted of air to such an extent that the pressure was reduced to one-millionth of an atmosphere. In some of his trial lamps he employed filaments made from Bristol board, but ultimately he chose filaments of bamboo as most suitable for the "Edison lamps."

There were experimenters in this field of incandescence lighting before either Swan or Edison took up the subject. M. Jobard, in 1838, suggested the use of carbon, placed in a vacuum, as a conductor, and this idea was carried out twenty years later, in 1858. Early workers were King, in our own country, in 1845, and Starr in America. Mr. Mattieu Williams was working with King and Starr, and he says: "We had no difficulty in obtain-

ing a splendid and perfectly steady light. We used platinum and alloys of platinum and iridium, and then tried a multitude of forms of carbon, including burnt cane."

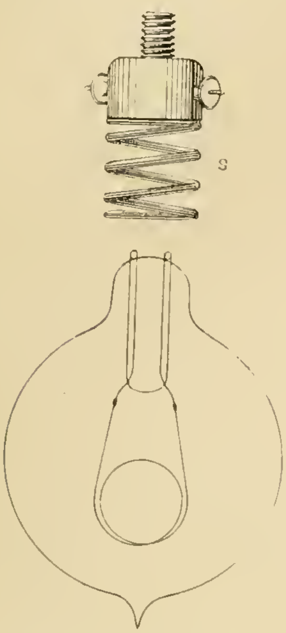


Fig. 2.

These attempts, however, were not successful in leading to any practical application on a large scale, and to Swan and Edison belongs the credit of first demonstrating the capability of this method of incandescence of carbon in high vacua to give permanent and satisfactory results in actual practice.

The following are the principal details in the structure of the glow lamps of Swan and Edison, and the points in which they differ. In the recent types of Swan lamp an organized material such as crochet cotton is taken; this is treated with acid, whereby the cotton fibres are reduced to a gummy consistency before they are carbonized. Edison starts with bamboo fibre, and has fixed on this substance because it possesses a definite structural form. The shape of the carbon in Edison's lamp is a long arch, or a carbon thread simply twisted back to run parallel to itself. Swan's form differs in having in the course of the carbon a spiral of one turn whose diameter is half an inch.

The thickness of Swan's carbon is about 0·25 millimetres, *i.e.*, $\frac{1}{100}$ of an inch. Edison's 16-candle power filament is oblong in cross section, the sides of the oblong being 0·1 and 0·2 millimetres, *i.e.*, $\frac{1}{250}$ and $\frac{1}{125}$ inches.

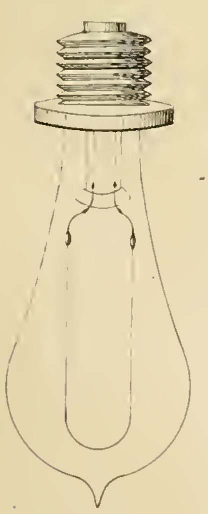


Fig. 3. — Edison's Lamp.

The filaments of carbonized bamboo in Edison's lamp are prepared thus:—The canes got from the plant, which are tubular in form, are prepared first of all by machinery devised for the purpose, so that a large number of uniform pieces is rapidly obtained. The canes are first of all sawn into suitable lengths. Each of these pieces is then split down its centre, and two half tubes are thus got; both of these are again divided into three strips, the hard outer rind containing silica is removed, and the stripped pieces are shaved down so as to produce a flat and straight strip of uniform thickness throughout its length; these strips are then cut so as to be of the same length. After a set of similar fibres has been thus prepared, they are placed in blocks and cut so that their final shape is a narrow strip of bamboo with enlargements at each end, by means of which they are joined to the connecting wires. The fibres are next bent into the required form, that of a loop or horseshoe, and carbonized by being raised to a white heat in muffles placed in a furnace. Next they are electroplated on to their platinum supports, this being done to ensure a thoroughly good contact, and then placed in their containing bulbs. These latter are repeatedly exhausted of air, whilst the fibre contained in them is again and again raised to a very high temperature (by the passage of an electric current) and allowed to cool down between its successive heatings. By this process the air or any occluded gas is got rid of, and besides this the fibres are subjected to such a severe test that only those which are quite sound can survive it, and in the end a fibre suited to the purpose in view and capable of long endurance is obtained.

The *efficiency* of different lamps is compared by finding the quantity of energy used up per candle power produced. This energy, which is consumed in the lamp, is equal to the difference of potential at its terminals multiplied into the current passing through it, and is generally expressed in volt-ampères, *i.e.*, the product of the potential (or electric pressure) expressed in the practical units or volts, multiplied into the strength of the current in ampères. Thus the "efficiency" of a glow lamp depends on the pressure at which it is worked and the strength of the current which circulates through it. The length of time which such a lamp lasts (or its "life") and its efficiency are intimately connected. The overlooking of this fact has led in the past to numerous misleading or partial statements, even in technical journals. To know the quality and goodness of a lamp we must be acquainted, not only with the number of hours it has existed while giving out light, but also with the amount of energy which has been employed in producing this light. As an example, I may take an instance given by Sir David Salomons. He says he has hundreds of lamps which have run over 3000 hours, and he attributes the long life of his lamps to his method of obtaining constant electro-motive force round his circuit. The pressure (or electro-motive force) in this instance is kept between the limits of 98 volts and 100 volts, and the results have exceeded expectations. Here on the average it has been found that 16-candle power Edison-Swan lamps, meant to be worked at a pressure of 100 volts, give a light whose intensity is equal to 15 candles at 98 volts and 17 candles at 100 volts.

For practical purposes, another important factor comes in, and one which determines which lamp shall be used in a given case, and this is the *cost* of the production of the energy required. This last factor varies with circumstances, the sources of power being so different—*e.g.*, waterfalls used to drive dynamo machines, secondary batteries previously charged up, primary batteries, &c.

The *commercial efficiency* of a lamp depends not only on the amount of energy required to work it, but also on the cost of this energy.

If we take a fixed quantity of gas and compare the result—*first*, when it is used to drive a gas engine which works a dynamo machine, and thus produces a current through a set of incandescence lamps, or, *secondly*, when the gas is burned at once, and thus used directly as an illuminant, it is found that the light-giving efficiency of the electrical method is three times that of combustion. But the cost of the electrical method is at present greater on account of the first expense, and then the maintenance of the gas engine and dynamo.

The advantages of using electricity as a means of lighting in mines are very noticeable. The only suitable lamps for such a purpose are those which are portable. Hence, if electricity is to be used at all, the lamps employed must be incandescence lamps. When these are made use of, the work becomes more sure, more regular, and more rapid, and it is even claimed that the moral nature of the miners is improved when they work surrounded by the brighter and more agreeable light of electric glow lamps. The

problem of the electric lighting of mines has not yet been thoroughly solved, but success seems to lie in the direction of a portable glow lamp worked by a secondary battery. This may be made very light, and the extra cost, which at the present moment is unavoidable, is more than saved by the preservation of life and limb and the altogether more satisfactory condition of the miner. Swan has invented an indicator which he attaches to his portable lamp. This depends on the observed fact that a red-hot platinum wire glows more brightly in an atmosphere charged with fire-damp than in pure air.

For intensity of illumination glow lamps can never compete with arc lamps, but the advantage of the former lamp is that they may be distributed at will, and concentration in one place, which is often a drawback, is avoided.

BEE PARASITES.—I.

By E. A. BUTLER.

THERE is no other order of insects that has made such strides in the cultivation of architectural and constructive talent as the Hymenoptera; and consequently, whenever the subject of insect skill is under discussion, the majority of the illustrations are sure to be taken from this order. Whether as excavators, stone-masons, carpenters, or workers in wax or in paper, many members of the Hymenoptera have made their mark in the world, and bees, ants, and wasps in particular have attained a high degree of excellence in one or other of these crafts. While the majority of insects are to a great extent wanderers on the face of the earth, having no definite spot with which their fortunes are inseparably associated, dwelling nowhere in particular, but, like nomads, living from hand to mouth, and prepared to take up temporary quarters wherever they may chance to find themselves at the end of a day's wanderings, most of the above-named have what may be more or less correctly called homes, fixed places of abode which they have carefully selected for the rearing of their families as well as for their own private residence, places in which they may be expected to be more or less continuously found, and upon which they often expend a vast deal of labour to fit them for the double purpose of a retreat for themselves and a shelter for their brood. The sense of proprietorship is thus developed, and its accompanying anxieties and responsibilities have to be faced, even if, as is often the case, only for the short period of a single summer season. The establishment of a home, and the accumulation of stores therein, creates a temptation to less thrifty beings to profit by the industry and skill that have achieved these results, and burglary and swindling thus become professions amongst insects as well as amongst mankind. It is in the existence of this, so to speak, landed proprietorship amongst the Hymenoptera, this elaborate construction of dwellings and advance in social habits, together with the dangers and risks consequent thereupon, that is to be found the reason for the numerous remarkable relations into which so many of them are brought with other insects. We have already seen good examples of this in the ants, and we now propose to take other illustrations of a somewhat different character from a kindred race. It is not with the hive bee that we shall concern ourselves; this insect is so artificialized through its close alliance with man, that it has no chance of furnishing such good examples as those species exhibit which have only their own cleverness and courage, unbacked by the assistance of superior beings, to depend upon in endeavouring to maintain their place in the world. Of these wild bees we have many British species, some of which—the humble bees—are social, while the rest are solitary. But both sections are alike subject to the attacks of thievish depredators, derived either from their own order, or from other groups, and the reason for the parasitism therefore is to be found, not so much in the development of the social instinct, as might perhaps have been expected from the example of the ants, as in the effects produced by architectural skill and forethought, the securing of a safe retreat and supplies of food, which serve as prizes to tempt the cupidity and appeal to the indolence of the unthrifty.

Bees are subject to the attacks chiefly of other bees and of flies, and the parasites in some cases closely resemble their hosts, but in others are quite unlike them. We may first consider the parasitism of the social bees. Apart from the hive bees, all British social bees belong to one genus, called *Bombus*. The word was coined by the ancient Greeks in imitation of the deep bass humming which these bees produce during flight, whence also their English names of humble or bumble bees. Some fifteen species of these insects are now reckoned as inhabiting the British Isles, and, like other social insects, they exist in three forms—males, females, and workers. Great differences in size appear in these. The females are by far the largest, and are the well-known great, heavy, loud-humming creatures that fly swiftly straight forward in a "bee line," or hum in a deep self-satisfied bass, on a sunny spring day, round the sallow bloom. The males are considerably smaller, and are generally very differently coloured from their mates, being either more brilliant or more variegated. The neuters are the smallest of all, and are usually more or less like miniature reproductions of the females, several sizes smaller. Their nests are not very numerous in individuals, and each owes its foundation to the exertions in early spring of a hibernated female, who is thus both the foundress and the widowed mother of the colony, her spouse having died the previous autumn. The first members of the colony are workers, and the grubs which produce them are fed with pollen and honey collected by the great queen mother. When they arrive at maturity, they undertake the work of the nest, providing food for the later members of the family. As the season advances, males and females are produced, which are destined to become the founders of next year's colonies, their mating taking place in the autumn, though the progeny is not produced till the next season. Such, in very brief outline, is the life-history of a *Bombus*. Now, there is a genus called *Psithyrus* or *Apathus*, superficially very much like *Bombus*, so much so in some species that a novice would be sure to regard them as the same insect. These are the parasites, and their economy is quite unlike that of the *Bombi*. They enter the nests of the latter, deposit their eggs there, and leave their offspring to be cared for and fed by the legitimate owners of the nest, while they themselves do nothing towards their maintenance. No labours, therefore, either in the collection of food, or in the construction or repair of nests, devolve upon them, and there is thus no need for workers as distinct from males and females, and hence we find that in this genus no such things as workers are known, there being only the two sexes as in solitary insects.

Further, as pollen collecting, to provide food for the young, is one of the chief occupations of the industrious bees, these parasites, having no need to undertake this labour, are unprovided with the necessary apparatus. The most important part of this apparatus consists of what is called the "corbicula" (little basket) (Fig. 1). The tibiæ of the hind legs of the female and worker

Bombi are broad and flat or slightly concave, and shining; no hairs grow on this shining surface, though all the rest of the body is densely covered, but a closely set row of long, stiff, curved hairs runs along each side of it, sloping backwards and slightly outwards, but not enough to prevent them from making a sort of trough-like enclosure, of which the polished surface of the tibia forms the bottom; this is the corbicula, or receptacle, in which the pollen is packed, after having been collected by means of the dense brushes of hairs which cover the body. The tibia of the males is sometimes nearly bare and polished, but its surface is somewhat convex, and the brushes of hairs at the sides spread out almost on a level with its surface, or are indiscriminately arranged, so that no enclosure is made. Now the *Psithyri* have no such arrangement as above detailed; the hind tibiæ of the female are not broadened, their outer surface is decidedly convex, not smooth and polished, but covered scantily with hairs, and no erect rows of bristles fringe their side margins, and those of the male are quite similar; these peculiarities alone are sufficient to distinguish these parasitic bees from their industrious hosts. The *Psithyri*, also, are not so densely covered with hairs as the *Bombi*: certain parts of the upper surface of the abdomen especially have the hairs short and thinly scattered, so that the bare shining skin can easily be seen through them, whereas the *Bombi* would have these parts covered completely with dense bands of fur, and scarcely any trace of the skin beneath would be visible.

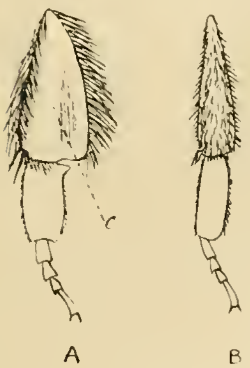


Fig. 1.—Left hind tibia of (A) *Bombus*, (B) *Psithyrus*. *c*, Corbicula.

Now, as these *Psithyri* in their larval condition are nourished at the expense of the working *Bombi*, and are therefore a constant drain on the resources of the nest, it is difficult to believe that the parasites would, if recognized, be tolerated in the nest unless they contributed in some way to the well-being of the establishment; and if they were at all conspicuously different in appearance from the true owners, it would be a risky matter to enter the nest, more particularly as they are connected almost entirely with those *Bombi* that are underground builders, and these are the most fiery-tempered of the genus. As they do not, so far as is known, contribute in any way to the advantage of the colony they are associated with, it is easy to conjecture a reason for the remarkably close imitation they exhibit of the appearance of their hosts, a disguise so complete that they are able to pass in and out of the nests entirely without molestation, a disguise, too, which appears to extend to the larvæ, for it is difficult to believe that the workers would consciously waste their time and efforts upon foundlings which in no way serve to strengthen or otherwise aid their own race. There is a curious point in connection with this imitative dress of the parasites that is worth notice. It is obvious that it is only the females and workers that any invaders would have to fear, for the males have no sting, and have therefore no means of attack except their jaws; the greatest safety would seem therefore to be obtainable by the parasites becoming undistinguishable from the dangerous groups of their hosts, while resemblance to the harmless males would not be so important. Hence we find that not only do the females of the *Psithyri* ape the colours of their female and worker hosts, but the males do so as well, thus becoming quite different from the same sex in the *Bombi*. Take, for example, the common red-tailed humble bee (*B. lapidarius*). The females and workers of this insect are generally black all over except the last three segments of the abdomen, which are bright red, highly suggestive of their fiery temper. But the male has, in addition to its red tail, a yellow-haired face, and a broad yellow band in the front of the thorax. Now the male of the *Psithyrus* which accompanies this species (*P. rupestris*) has no yellow, but, like the female, is usually coloured only black and red, with at most a few greyish patches at the sides. The resemblance between the two females, however, is not quite so close as a description of their bodies alone would lead us to imagine, for the wings of the guest are much darker than those of the host. The resemblance between the other species of *Psithyri* and their hosts is perhaps not quite so close as in the case indicated above, but still sufficiently so to cause them to be confounded on a casual glance; examination of the hind legs would at once show their distinctness. In size they usually compare with the male and female *Bombi*, and are therefore larger than the workers. There may be another reason for the imitation of the female and worker *Bombi*, rather than of the males, which may be found in the fact that the males much less frequently visit the nest after they have once left it, and therefore the parasites would become more noticeable if coloured like these occasional visitors, than when resembling such constant residents as the workers.

The nests of the red-tailed bee (*B. lapidarius*), are frequented by another parasite whose relations to its hosts are much more serious. This is a two-winged (dipterous) fly, called *Volucella bombylans*. It is something like a large bluebottle in shape, and about the size of the worker bees, which it imitates in colour, being black-bodied and red-tailed. Entering the nests it lays its eggs there, and the maggots issuing from these devour the larvæ of the bees, sometimes destroying almost the whole brood. It seems marvellous that these depredations are not checked by the bees themselves, which apparently have the remedy in their own hands, since they might easily sting the larvæ to death, even if the parents had escaped notice. One almost feels disposed to conclude that they fail to discriminate between the fly maggots and their own larvæ, though there is a considerable difference between the two. A similar apparent obtuseness of perception is exhibited with regard to the flies themselves; for the differences between the two-winged and light-bodied fly, and the four-winged and much more substantial bee are sufficiently obvious, notwithstanding the disguise; and it is scarcely credible that if they did recognise the flies, and divine in the least degree the object of their visit, they would allow them to enter the nest unmolested. On the other hand, if they really do not distinguish between the flies and their own comrades, the necessity for the imitative dress seems to be rather less obvious. It is generally maintained that bees recognise their comrades belonging to the same nest, but several observers have suggested that this is by smell rather than by form or colour, in which case strange flies would seem to be placed at a disadvantage, since they evidently would not possess the smell peculiar to the nest they might wish to enter. Sir John Lubbock's well-known experiments serve to show that the colour sense is well developed in bees, but this does not imply an equal power of perceiving details of form, and hence it may possibly be sufficient for the parasites to show a black and red body in order that they may pass muster. The risk run by these flies, however, on the supposition that the bees really do object to the destruction of their offspring, and would fight in their defence if they knew

of the danger to which they are exposed, must be greater than in the case of the parasitic bees already referred to, since not only is theirs the less venial offence, but if it came to a fight, the *Psithyri* have their stings to defend themselves with, and would no doubt give a tolerably good account of themselves, while the flies have absolutely no means of defence when once their disguise is penetrated, and would be quite at the mercy of their foes.

Volucella bombylans does not confine its attentions to the red-tailed bee, but visits the nests of the yellow-tailed ones as well, and it is a remarkable fact that the colour of the parasite varies with that of its host. Those individuals that live with the red-tailed bee are themselves red-tailed, but those that come from the nests of the yellow-tailed ones themselves partake of this style of ornamentation, and show no red at all; so different in fact are these varieties that it is difficult to believe that they represent the same species. Other species of the same genus also occur in bees' nests, but in these cases there is not the resemblance between parasite and host that obtains in the instance already cited, and yet the flies seem to be able to elude observation and carry out their piratical enterprises in safety and with success. These flies also inhabit wasps' nests, where they destroy the brood; in this case there is certainly no attempt to imitate the colours of the hosts, but then, according to Sir John Lubbock, wasps have the colour sense less well developed than bees, and therefore possibly any detailed imitation might only be thrown away upon them.

Another parasite to which some humble bees are a prey, is an insect not very commonly found in this country, and sometimes called a solitary ant. Its scientific name is *Mutilla Europæa* (Fig. 2). It consists of males and females only, the former of which are winged as usual, but the latter entirely apterous. The thorax is bright red, and the abdomen, the ground colour of which is black, is prettily banded with thin lines of glittering yellow hairs. Thus it is quite unlike the bees in shape and appearance, and the female looks more like an extremely large and very strong and stout ant. This insect is carnivorous in its habits, and its larvæ devour those of the bees, sometimes, in countries where they are plentiful, destroying almost the whole brood; in this country they are never sufficiently numerous to do this. That they are neither essential parasites of the *Bombi*, nor entirely confined to those insects is clear from the facts that though the *Bombi* are extremely common insects with us, the *Mutillæ* are rather rare, and there must be hundreds of nests of bees from which they are absent, and that, on the other hand, *Mutillæ* either of this species or others closely allied are often abundant in countries where *Bombus* is either rare or does not exist at all. Other insects also may be found in the nests of social bees, such as little beetles and mites. The former come in for the sake of the wax, with which the honey-pots of the bees are made, and the latter partly for this too, but also to attack the bees themselves, to whose bodies they cling, and whose juices they suck, exhibiting thus a third type of parasitism. In the other instances we had the parasites attacking either the food of the young bees, or the larvæ themselves; now we have parasites which subsist upon the perfect insects.

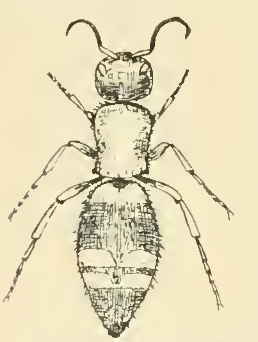


FIG. 2.—*Mutilla Europæa*, a parasite of humble bees, magnified three diameters.

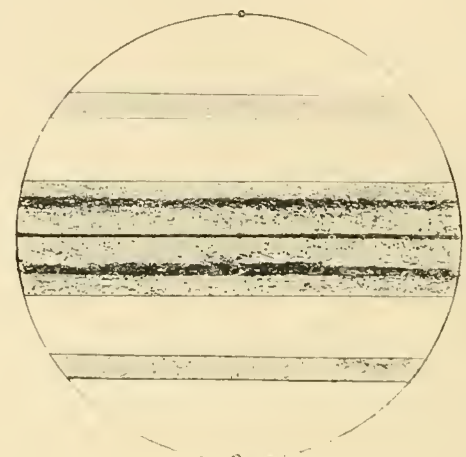
(*To be continued.*)

THE DISTRIBUTION OF SUNSPOTS IN SOLAR LATITUDE.

By E. W. MAUNDER, F.R.A.S.,

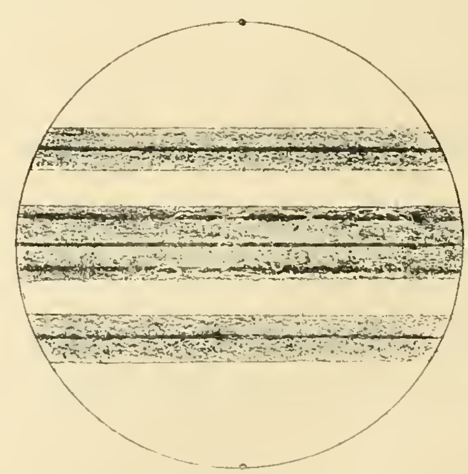
Assistant superintending the Solar and Spectroscopic Departments at the Royal Observatory, Greenwich.

TO the attentive observer of solar phenomena, the vast size, beautiful detail, and changes of form of the great group of February last did not constitute its only, perhaps not its chief claim to notice; and even its association with the great magnetic storm of February 12th, and with the appearance of auroræ, is scarcely of more importance than the feature to which I wish now to allude—I mean its drift in solar latitude.



Just before Minimum.

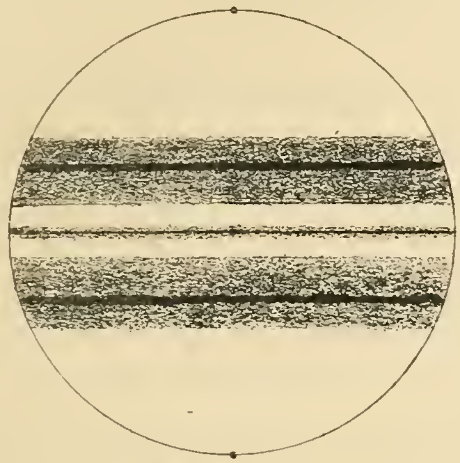
Briefly the history of this great outburst is as follows: A considerable group was observed in S. lat. 19° during November, 1891, and it was accompanied by two companion groups, which for the sake of distinction we will call B and C, denominating the principal group as A. B was in S. lat. 19°, and C in S. lat. 11°. During December C was not observed, but A and B were; the former in S. lat. 20°, the latter in S. lat. 19°. During this rotation B became the more considerable group, and in the next



Just after Minimum.

rotation, that of January, 1892, it was seen alone; this time in S. lat. 25°. During February, when it attained its greatest development, its latitude was 27·5°, and in March, when it was much smaller in area, it was a little

further south still, viz., in S. lat. 28·5°. It had shown, therefore, a steady movement away from the equator from November, when it lay in S. lat. 19°, to March—the month when it was last seen—when it was very nearly 10° further south.



Before Maximum.

We have to bear in mind that this implies a drift of more than 70,000 miles in less than four months—indeed, in one month of the four, the drift amounted to 46,000 miles—and that it concerned an object which, whatever its nature, showed on the average an area of 600 millions of square miles, and a maximum area of six times that amount.

As the February group was the greatest in area of all those observed during the period covered by the Greenwich record, so it stands out as pre-eminent in its drift. But other groups also have shown a great and remarkable drift; indeed, a large and active group is almost sure to display some motion of the kind, and in many instances the evolutions performed by a large group are of the most complicated character.

If, however, we confine ourselves to the great groups to which we have already drawn special attention in earlier papers, viz., those of April and November, 1882,

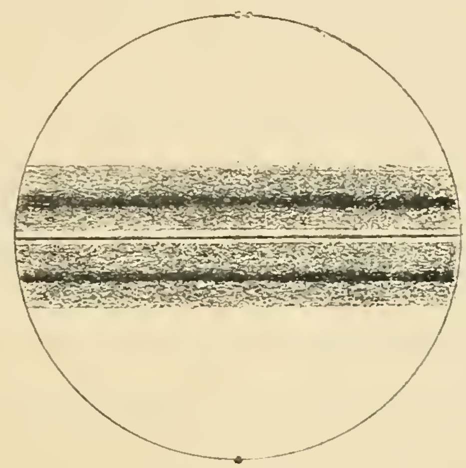
At Maximum.

we find that the April group showed a drift of $6\frac{1}{3}°$ in latitude during a single month; slightly greater than the maximum drift of the recent spot, but as the group of April, 1882, was only observed during three rotations, it did not show that long persistency in southward motion which the later group has done. The second great group of April, 1882, was only observed during one rotation. The third great group of 1882, that of November, showed little or no persistent drift, remaining almost stationary for the greater part of the three months during which it was under observation, but displaying a rather rapid movement towards the equator during the last four or five days of its existence.

A yet more curious feature of this drift in latitude, which is frequently to be noted, is the tendency to return to the latitude from which the group first started. A single example may illustrate the point.

The group of August 2nd, 1889, formed in S. lat. 21°, and steadily moved towards the equator, being last seen in its first rotation in S. lat. 20°. When it was seen at the east limb again on August 27th, its latitude was 19°. It steadily moved upwards during its appearance, and had nearly regained its old position of latitude 20° before it passed out of view at the west limb. During its third and last appearance it oscillated between 21·5° and 20·5°, finally disappearing when in the last-named latitude.

Drift in latitude is therefore not at all an uncommon feature of sunspots, though drift to so large an extent as that shown by the great group of February last, viz., 10° in four months, is most exceptional. Yet such



After Maximum.

drift is not at all what we should have naturally expected. For there is a well-marked difference in the speed of rotation of the various zones of the solar surface. Spots on the equator, for instance, not only have an actual rate of speed higher than those north or south of them, but a more rapid angular motion; in other words, whilst a spot on the equator takes 25 days to complete a rotation, one in latitude 36° takes 27.

But curious as are these instances of motion in latitude in the case of specific groups, there is a far more curious phenomenon, akin to it, shown by sunspots as a whole—I mean the change in the latitudes affected by spots at various stages of the sunspot cycle.

Let us suppose that we are watching the sun during the period of decline after maximum; during such a time as that covered by the years 1886 and 1887, for example. What should we notice? In 1886 we should notice that spots were seen over the entire belt from 20° N. to 20° S., not a few lying quite close to the equator, their mean position in either hemisphere being about 10° of latitude; but that out of 128 separate groups only one, and that a very small one which lasted but two days, was seen further from the equator than lat. 20°. In 1887 the tendency was to approach the equator still closer. The mean latitude of the entire spotted area was 8·5°, and no single group attained a distance from the equator of

20°. Indeed, of 79 groups only four were as far from it as lat. 18°. Spots in their "teens" were becoming decidedly scarcer, whilst spots on or close to the equator were relatively more common.

It will be noticed that this change in position accompanied a marked decline in the numbers and areas of the groups. In 1886 there were 128 groups observed, and the average daily extent of the spotted area of the sun was 450 millions of square miles. In 1887 there were only 79 groups, and the average daily extent of the spotted area had shrunk to 210 millions of square miles. In 1888 the number of groups had gone down to 54, and the mean daily spotted area to just half what it had been the previous year; and this shrinkage had been caused for the most part by the disappearance of high latitude spots, so that the mean latitude of the spotted area was now only 7⅓°. North of the equator, the only group that exceeded 10° in latitude lay in latitude 11°; in the southern hemisphere, which was the more active, there was one group at 12°, two at 13°, and one at 15°, but all the others were under 10°; so closely were such groups as still appeared huddled together towards the equator.

But on the last day but one of the year, one little faint insignificant spot broke out in quite a different region from these. True, it was gone before the morrow. Its mandate might have been like that of Macbeth's visions, to "come like shadows, so depart." But none the less, small, faint, and fleeting as it was, it was the harbinger of a mighty change in the general aspect of the sun.

The next year, 1889, showed a yet further decline in the solar activity. Only 32 groups were observed, and the mean daily spotted area was now only 90 millions of square miles; and during the first half of the year, so marked was the decay of the high latitude spots, that not one instance was observed of the mean position of a group exceeding 10°.

But on June 29th the harbinger spot of the previous December was followed by a second, quite as small and faint, and nearly as evanescent, for it lasted only two days, but in a higher latitude still, 40·3°. Other groups followed it. A group was seen in S. lat. 24° in July; a fine group in S. lat. 20° ran its course during three several appearances, in August, September, and October; and in all, counting these three apparitions of this group as three different groups, twelve groups were seen with latitudes exceeding 20° during the last half of the year, whilst nine appeared in the equatorial region. The spots during the last six months of 1889 were therefore divided into three distinct zones—a northern zone, comprising spots with latitudes of 20° and upwards; an equatorial zone, embraced between latitudes 10° N. and S.; and a southern zone, with spots of latitude 20° and upwards. But the zone 10° to 20° on either side of the equator was wholly barren.

In 1890 the sun began to show clear signs of reviving activity. With this revival of activity came a further decline in the number and area of spots in the equatorial zone, and a very rapid increase in those of high latitude. Only four groups out of the fifty-five were found in the former region, and these were but small. With October, 1890, the old series practically came to an end, and since then the high latitude spots have held the entire field. Several groups have been seen between 30° and 35°, and two out of every three groups were situated in the 20° to 30° zones.

But as time goes on, these tend ever to seek lower and lower latitudes. In 1891 there were two spot zones, one north and the other south of the equator, and the equatorial region was entirely barren; but as the cycle progresses and as spots in positions as high as 28° or 30° become rarer, so spots close to the equator become more common, till at length the state of affairs will again be as it was in 1886 and 1887, and there will be practically but one spot zone, and that close to the equator.

We have, therefore, three phases of the spot-cycle. I. Before minimum; spots few and diminishing; one spot zone, viz., that close to the equator. II. After minimum; spots few but increasing; three spot zones, viz., an equatorial zone, and a zone of high latitude in either hemisphere, separated from the equatorial zone by a broad barren belt. III. At or near maximum; spots numerous; two spot zones, one north and one south of the equator, the equatorial region itself being entirely barren. It follows, then, that the two barren belts of the second period are on the average the most prolific for the cycle as a whole, for they become the seats of spot activity just at the time when sunspots are largest and most numerous.

There is, therefore, a steady and continuous tendency ever to seek lower and lower latitudes from the beginning of a cycle till its end. The first small and faint spots of the new cycle break out in the very highest latitudes attained. In this sense the new period begins at once with its utmost vigour, and during the years in which it runs its course is simply undergoing a continual decline. As to the numbers and areas of the spots, the case is very different. For some three years there is a most marvellous and rapid increase, followed by two or three years of great activity. Then comes a decline, which is, however, slower and less striking than the increase. Finally, for about a year, we have on the sun the last feeble relics of the old cycle, together with the first heralds of the new one. Or we may put it another way. The sunspot cycle is about eleven years on the average, but each particular spot-cycle takes about twelve years to entirely run its course, so that for a year the two cycles run concurrently.

The following little table will illustrate this for the last spot-cycle, that of 1879–1890:—

Year.	Mean Daily Area.	Mean Distance from Equator of all Spots.	Year.	Mean Daily Area.	Mean Distance from Equator of all Spots.
1879	54	23·57°	1885	957	11·76°
1880	491	19·80°	1886	450	10·38°
1881	861	18·21°	1887	211	8·44°
1882	1182	17·81°	1888	105	7·39°
1883	1363	13·04°	1889	61	6·34°
1884	1273	11·27°	1890	6	7·14°

The mean area is expressed in millions of square miles, and for the years of overlapping, 1879, 1889, and 1890, only the spots proper to this cycle have been included.

This was no special behaviour of a single cycle. It is the usual typical behaviour. If we take the cycle 1856–1867, and divide it into periods of fifteen rotations on the average, we find the mean spot latitude of the successive periods will run as follows*:—32°, 26·7°, 21·3°, 17·8°, 17°, 14·2°, 12·1°, 10·4°, 10·3°, 9·2°, 8·2°, 8°. Of these twelve periods the first two were coincident with the last two periods of the earlier cycle, with mean latitudes 7·8° and 5°; and the last two, with the earliest periods of a new cycle, latitudes 31° and 22°. Similarly, this cycle, 1867–1878, ran from 31° and 22° to 22·6°, 18·3°, 16·2°, 13·8°, 11·2°, 11·4°, 10·3°, 9·2°, 7·0°, and 5·4°; and these last two periods again were coincident with the first two of the cycle of which we have already treated, that of 1878–1889, as the last two periods of that cycle in its turn were coincident with the first two of the cycle through which we are now passing.

* The figures are those of Dr. Spoerer (*Astr. Nach.*, No. 2565), who more than any other observer has brought these relations under the notice of astronomers.

North.

THE REGION OF THE MILKY WAY ABOUT β CYGNI.

Enlarged from a Photograph taken by Dr. MAX WOLF, of Heidelberg, with a Kranz Aplanatic Camera of $5\frac{1}{4}$ inches aperture and a focal length of $30\frac{1}{3}$ inches; exposure 12 hours.

The accompanying diagrams, in which an attempt is made to represent the manner of spot distribution at different periods of the cycle, will perhaps set forth these changes more clearly than much description can do.

So strongly marked a relation must evidently be given great weight in any theory which we form as to the origin and nature of sunspots. Unfortunately, I cannot claim to have framed any such theory myself, and the only point I would urge is that these various relations—the sudden appearance of the spots of a new cycle in high latitudes, the persistent decline in latitude of the general spotted area as the cycle progresses, and the drift in latitude of individual groups—seem to me absolutely fatal to the idea, once popular, that the secret of solar disturbances lies without the sun; in the relative positions of the planets, for example, or in the fall of meteorites.

If this purely negative conclusion is felt to be disappointing, I cannot help it. I feel no mission myself to explain everything, but rather prefer to state the facts as I know them for others to explain. Yet it is no light matter in our progress towards truer theories that we should get rid of the false ones.

I think, therefore, we may settle ourselves that the secret of the sunspot problem lies below the solar surface, not above it; and the well-known fact, that outbreaks often recur in the same districts after considerable intervals of time, appears to me to strongly confirm it.

There is one other circumstance, in connection with this decline in latitude as the spot-cycle progresses, to which I should like to allude. Just as the increase in the number and size of spots after minimum does not proceed regularly, but by waves, one fortnight being rich in spots, and the next poor, and so on, oscillating to and fro, but the days without spots becoming ever fewer, and the days with spots both more numerous and richer, so it is with the decline in latitude. It proceeds in waves. Every fourth or fifth rotation there will be an effort to reach a higher level, a lift of one or two degrees, and then a gradual slipping back until a fresh effort brings another small lift, but a weaker one than the last. And so the cycle goes on; the decline is continual on the whole, but is broken and interrupted by these frequent little struggles to get back to a higher plane. One could almost fancy one saw a living being trying to maintain a losing battle with ever-declining strength. No doubt these minor oscillations proceed from a cause similar to that which gives rise to the great lift in latitude at the beginning of a new cycle, and it is in the attentive study of the laws according to which these changes occur that we may hope to unravel their meaning, and in so doing add to our knowledge of the constitution of the sun.

WHAT IS A NEBULA?

By A. C. Ranyard.

THE plate which illustrates this paper has been made from a photograph kindly sent me by Dr. Max Wolf, of Heidelberg. It represents a region of the Milky Way not very distant from the regions represented in the plates published with the October and December numbers of Knowledge for last year.

The bright star near the centre of the plate is β Cygni, a beautifully coloured double star well known to the possessors of small telescopes as a very easy test object. It is not shown as double in the plate because the photographic trace left by the two stars is a patch, or disc, more than a tenth of an inch in diameter, which corresponds to some seven or eight minutes of arc upon the heavens, while the two stars, though they form a pair which may be easily split by a good opera glass, are only 34″ apart—that is, on the scale of the plate their centres would be separated by a distance of only about $\frac{1}{150}$th of an inch. This minute separation, though not recognizable on the photograph, corresponds to a real distance in space which is so great that during the time in which man has been observing them no motion of one star about the other has been noted, though their colours would lead us to conclude that they are really associated. The larger star, which is of the third magnitude, shines with a golden yellow light, while the smaller star of the seventh magnitude is a beautiful azure blue, thus conforming to the general law that in coloured binaries the smaller star is always bluer than the larger one.

This plate of Dr. Max Wolf's, although it was exposed for twelve hours, does not show as much nebulosity as the plates of the regions around α Cygni and ξ Cygni, published in the October and December numbers of Knowledge; but especially in the lower half of the plate there is a very distinct background of nebulosity, interrupted here and there by dark channels and dark prominence-like forms, associated with lines of small stars.

The nebulous background of the Milky Way is too faint to allow of its light being analysed with the spectroscope, but its whitish colour, as seen with the naked eye, seems to indicate that it does not belong to the type of nebula which gives bright lines or a gaseous spectrum.

About half the nebulæ that have been examined spectroscopically give a spectrum in which six or seven lines are fairly conspicuous. The three brightest of these lines are situated in the green,* and they give to this class of nebula a very distinct bottle-green tint that enables an observer with a large telescope to recognise one of these gaseous nebulæ at sight as differing from a white nebula, such as the nebula in Andromeda, which gives a continuous spectrum unmarked by any well-recognizable lines or bands.

The faint continuous spectrum of the white nebulæ is not crossed by any dark absorption lines such as we see in the spectra of the stars. If the light of these nebulæ were due to thinly scattered faint stars, too small to be individually visible, we might expect their combined light to give a faint spectrum crossed by dark absorption lines common to the spectra of the small stars. That such absorption lines are not visible in the spectra of the white nebulæ is *primâ facie* evidence that they do not consist of sparsely distributed bodies similar in constitution to the brighter stars.

It has been assumed that the greenish nebulæ which give bright line spectra are masses of incandescent or glowing gas; but such masses of gas, if quiescent in space, would be cooler on their outsides, and the outer cold layers would, according to the theory of exchanges, absorb the radiations given out by the glowing gas within. The bright line spectrum of these nebulæ seems always to be accompanied by a more or less faint background of continuous spectrum, such as would be given out by glowing solid or liquid particles, or by gas under pressure. Reasoning from first principles, it seems probable that a quiescent mass of hot gas cooling in space would give out a continuous spectrum from its lower regions, where it is under pressure, so that the free paths of its molecules are relatively short, and this faint continuous spectrum would, it seems probable, be channelled by dark lines in places

* All the gaseous nebulæ give approximately the same type of spectrum. The brightest line is situated in the green region, at wave-length 500·4; the next in brightness is also in the green, at wave-length 495·8; and the next in brightness is the well-known bluish-green F line of the hydrogen spectrum situated at wave-length 486·1.

corresponding to the wave-lengths absorbed by the outer cool gas. If, however, a part of the gaseous constituents condensed into liquid or solid particles in the outer and cooler regions of the nebula, we should have the above conditions complicated by the action of the glowing particles on the molecules of the uncondensed vapours surrounding them.

The extreme faintness of the nebular glow is a fact that must not be lost sight of in seeking for a possible explanation of the facts observed. If the nebular matter glowed with a millionth part of the brightness of the solar photosphere, the nebulæ would give us far more light than they do.

Let us assume with Prof. E. C. Pickering that the stellar magnitude of our sun is –25·5—in other words, that our sun gives about forty thousand million times as much light as a star of the first magnitude. According to this estimate, the sun would need to be removed to a distance where its diameter would appear to subtend an angle of only ·00964 of a second of arc to reduce its light to equality with that of a star of the first magnitude. A nebula which subtends a minute in diameter and gives the light of a star of the eigth magnitude gives $\frac{1}{631}$th part of the light of a star of the first magnitude, while its diameter is more than six thousand times as great as the diameter which our sun would appear to have at a distance where its light would appear equivalent to that of a star of the first magnitude; consequently the solar photosphere must, area for area, be more than twenty-two thousand million times as bright as such a nebula.

According to Prof. Langley, the sun's photosphere* is 5300 times brighter than the molten metal in a Bessemer "converter"; consequently, the nebula must glow with a light which corresponds to less than one millionth of the brightness of white-hot iron. Perhaps one might compare the brightness of the nebula with the faint glow of the trail left by a large meteor on entering our air. Such trails seem to be due to glowing red-hot particles. The vapour driven off from the meteor is for an instant intensely luminous. It is no doubt under great pressure, and rapidly expands, driving back the cold upper air. In expanding, it cools till the greater part of the vapour is precipitated into a glowing mist, which having no elasticity, and not being able to do work by driving back the surrounding air, can only cool by radiation. In this condition the trail from a large meteor sometimes remains faintly glowing for half an hour. The evidence seems to show that at first the trail gives out bright lines in addition to a continuous spectrum. The study of such trails may throw much light on the constitution of the nebulæ.

* See Prof. C. A. Young's "Sun," p. 245.

Letters.

[The Editor does not hold himself responsible for the opinions or statements of correspondents.]

THE EVOLUTION OF DOUBLE STARS.

To the Editor of KNOWLEDGE.

SIR,—I am not sure that I quite follow the theory of Mr. See on the evolution of binary stars, but I think generally he maintains that these stars owe their highly eccentric orbits to the fact that their masses are much more nearly equal than, for instance, those of the sun and Jupiter. Assuming this to be the case, we would expect the orbits of very unequal double stars to be much less eccentric than those of nearly equal ones; and although the mass of a star may not correspond with its magnitude, there must on an average be a pretty close relation between them. I accordingly examined the stars in Mr. Gore's catalogue, taking in each instance the latest orbit as founded on a larger number of observations than its predecessors. I classed as equal the binaries whose magnitudes did not differ by more than one-half, and as unequal those whose difference was not less than two magnitudes. The following is the result of my comparison:—

UNEQUAL BINARIES.		EQUAL BINARIES.	
Star.	Eccentricity.	Star.	Eccentricity.
η Cassiopeiæ	0·6244	ρ Eridani	0.674
40 Eridani	0·1362	Σ 1037	0·632
OΣ 149	0·460	Σ 3121	0.3086
Sirius	0·4055	OΣ 215	0·4346
α Centauri	0·5443	OΣ 234	0·3629
γ Coronæ B.	0·3483	γ Virginis	0·8715
λ Ophiuchi	0·4424	42 Comæ	0·480
ζ Herculis	0·463	η Coronæ B.	0·2667
70 Ophiuchi	0·4994	OΣ 298	0·5836
99 Herculis	0·7928	Σ 2091²	0·7352
δ Cygni	0·327	μ Draconis	0·493
β Delphini	0·0962	ζ Sagittarii	0·1698
τ Cygni	0·3475	γ Coronæ Aust.	0·4244
OΣ 489	0·343	ζ Aquarii	0·6000
85 Pegasi	0·35	Σ 1819	0·3052
25 Can. Ven.	0·7221	Σ 2173	0·1349
		ξ Scorpii	0·0768

No striking contrast will be found here, nor do either set differ much in eccentricity from those where the difference in magnitude between the components ranges between one-half and two. The eccentricities, however, are in most cases very uncertain. Thus in Mr. Gore's table those for Sirius range from 0·4055 (Mr. Gore's) to 0·945 (Mr. Mann's), for Castor from 0·300 to 0·797, for α Centauri from 0·5260 to 0·9689, σ Coronæ Borealis 0·3088 to 0·7515, λ Ophiuchi 0·4424 to 0·8191, τ Ophiuchi 0·0375 to 0.6055, &c. The following, however, are agreed in either by all computers, or a large majority: γ Virginis (equal) 0·85 to 0·9, ξ Ursæ Majoris (medium) 0·38 to 0·43, λ Ophiuchi (unequal) 0·44 to 0·5, ζ Herculis (unequal) 0·43 to 0·48, 70 Ophiuchi (unequal) 0·4 to 0·5, γ Coronæ Borealis (medium) 0·34 to 0·39, Σ 3121 (equal) 0·26 to 0·38. The result, on the whole, does not seem to me to favour the theory of tidal evolution.

Truly yours,

W. H. S. MONCK.

P.S.—In the solar system the rule appears to be that the orbits of the smallest bodies are the most eccentric. It may be worth noticing that the equal pairs (with computed orbits) appear to have in all cases spectra of the second or solar type.

[By courtesy of the Editor I have been permitted to read Mr. Monck's criticism, and will say in reply that he slightly mistakes my meaning. Large relative masses imply the development of large eccentricities only in so far as these masses imply large initial moments of momentum of axial rotation. For upon the moments of momentum of axial rotation depends the increase in the orbital momentum and mean distance, with which the increase of the eccentricity is so intimately connected. Unless the sum of the moments of momentum of axial rotation of the stars of a system is greater when the stars are equal than when unequal, it does not follow that the eccentricity would become higher. According to theory, therefore, we must not conclude that (for example) η Cassiopeiæ (masses 1 to 4) should have a smaller eccentricity than α Centauri (masses equal). It is also to be borne in mind that the eccentricity will depend

upon the age of the system. For a more complete discussion of these questions I must refer Mr. Monck to my paper on the "Evolution of the Double Star Systems," which will appear in November, and will probably remove any doubt that may remain as to the correctness of the theory of double star evolution. Numerical calculation shows that tidal friction is an amply sufficient cause to account for the great eccentricities, and also that the eccentricity developed under different conditions would be very different. It is not, therefore, surprising that eccentricities of every kind exist, as this could have been predicted from theory.

Very faithfully yours,

Zimmer Straza, Berlin. T. J. J. See.]

A MEAN-TIME SUNDIAL.

Most people know that an ordinary sundial does not give clock time—sometimes the dial time is fast and sometimes it is slow as compared with clock time; for sundial days are not, like ordinary days, all of equal length. When the earth is in the part of its orbit which lies nearest to the sun, it moves faster, and describes a greater angle about the sun in twenty-four hours than when it is in the part of its orbit furthest from the sun; consequently, when the earth is in perihelion the solar day exceeds the sidereal day by more than the average amount, and the shadow of the gnomon comes round again to twelve o'clock a little later than when the earth is in aphelion.

The mean time shown by ordinary clocks is based upon the division of a mean day, which corresponds to the mean or average length of the day as measured by the sundial shadow at different parts of the year. The clock time corresponds to the dial time which would be shown by a "fictitious" or "mean" sun moving uniformly in the equator at the same average rate as that of the real sun in the ecliptic.

The "equation of time" corresponds to the difference of time which would be shown on a dial by the *real* sun and the *mean* sun. It is reckoned as *plus* when the sundial is slower than the clock, and *minus* when it is faster. It is the correction which must be applied to the ordinary dial time in order to obtain mean time, and it sometimes amounts to more than sixteen minutes. Although a great deal of ingenuity and thought has for centuries been expended upon the construction of sundials, I am not aware that any one of the old dial makers ever succeeded in contriving a dial to show mean time. The difficulty has, however, at last been overcome in a very simple manner by Major-General J. R. Oliver, who has devised a gnomon which practically makes its own correction for the equation of time.

The peculiarity of the instrument is that the time is indicated, not by the shadow of a straight edge, as in the old sundials, but by the point where an equatorial circular line is cut by the edge of the shadow of a curved surface, the curvature of which is so arranged with respect to the sun's distance above or below the equator as to compensate for the "equation of time."

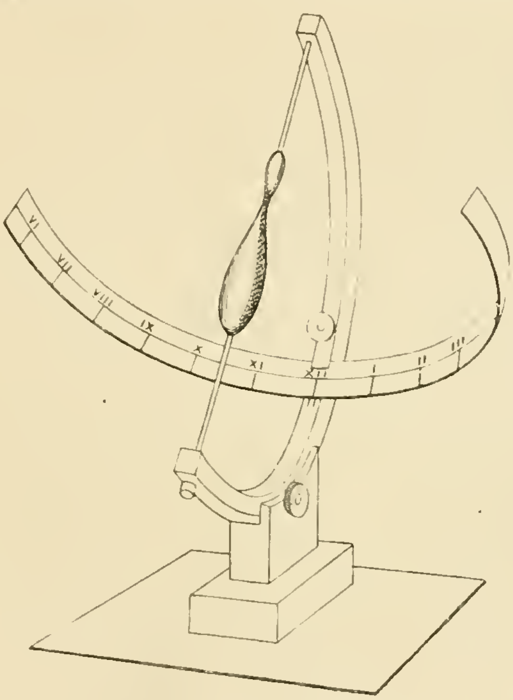
The instrument, says Major-General Oliver, is a universal one, and consists of a meridianal semi-circle, the diameter of which is an axis carrying the curved gnomon, and an equatorial circular arc. The latter has engraved upon its concave surface a graduated line, on which are marked the hours and their subdivisions. There is a screw for clamping the meridianal arc at the proper position for any given latitude, and another clamp for adjusting the equatorial arc.

The dial not only indicates local mean time, but by a very simple adjustment may be set so as to show any required standard time. Thus it might be set at Plymouth to indicate Greenwich time.

Strictly speaking, there ought to be two gnomons, one to be used from June to December, and the other from December to June; but by adopting a gnomon of mean contour the greatest error introduced at any time is only about one minute, an amount not more than the unavoidable one due to the softness of the edge of the shadow.

Four times a year the equation of time vanishes, and the gnomon would then intersect its own axis. To allow for the necessary thickness of the latter, a slight adjustment of the hour circle is necessary at these times.

Although the invention is little more than a scientific toy so far as England is concerned, it is believed that it would be of great use in countries where sunshine is plentiful and means of regulating the time are scarce. South Africa, South America, Australia, and India may be adduced as instances. In India, for example, there are numerous up-country stations at a distance from any railway, where there are practically no means of correcting clocks and watches.



The figure shows the form of the dial devised by Major-General Oliver. A comparatively cheap mean-time sundial might, I would suggest, be easily made by amateurs, with a glass globe such as is used for keeping gold-fish. A divided circle would need to be etched or painted round its greatest girth, and the gnomon might be made of a knitting needle centrally placed and carrying a piece of wood or gun-metal turned to scale from a curve which may be laid down from a table (such as one finds in *Whittaker*) giving the equation of time for each day of the year. The knitting needle should be passed centrally through a cork or bung in the mouth of the globe, and into a block fixed by marine glue at the bottom of the globe; and the globe must then be tipped on one side and supported in a stand so that the knitting needle is parallel to the earth's axis. From December 25th to April 15th, when according to the almanacs the sun is after the clock, one reads the time from the following edge of the shadow. When the equation of time has vanished and the

Sun is before the clock, one reads from the preceding edge of the shadow of the club shaped bob on the gnomon, at the place where its shadow cuts the hour line.

One of Major-General Oliver's dials may be seen at Messrs. Negretti and Zambra's shop on the Holborn Viaduct. A. C. Ranyard.

THE FEEDING OF A PLANT.

By J. Pentland Smith, M.A., B.Sc., *Lecturer on Botany at the Horticultural College, Swanley.*

PLANTS take in their food material from two sources—the soil and the air. The manner of its absorption depends upon the constitution of the plant under consideration. So far as regards the nourishing process, the vegetable kingdom may be divided into two large classes—the first including all those plants which possess green colouring matter, the second those in which this green substance is conspicuous by its absence. Our present consideration will be the feeding of green plants, and we will preface our remarks thereon by referring generally to the structure of these, and specially to that of the higher members of the series.

The green plant detachment numbers in its ranks the most simple structures and the most complex organisms. The simplest are composed of a single sac or bag, which we call a *cell*, and the highest members of the series are built up of an aggregation of cells. "The true meaning of the word 'cell' may be quite clear to but few, the less so since biologists themselves, even now, hold and discuss the most different opinions upon it. To many, the cell is always an independent living being, which sometimes exists for itself alone, and sometimes 'becomes joined with' other millions of its like, in order to form a cell-colony, or, as Hæckel has named it for the plant particularly, a cell-republic. To others, again, cell-formation is a phenomenon very general, it is true, in organic life, but still only of secondary significance; at all events, it is merely one of the numerous expressions of the formative forces which reside in all matter, in the highest degree, however, in organic matter."*

Much has been written on the subject of the plant structure, and the subject itself is an exceedingly interesting one to the anatomist; but to the general reader, at least, anatomy, apart from physiology, is a dry study. The two departments ought always to go hand in hand.

In the lowest plants, and in the lowest animals, a single cell performs all the life-functions. As we proceed higher in the scale of organization we notice that differentiation takes place, and continues until, in the highest forms, a complex series of tissues exists, each fitted for the performance of a definite work. In both kingdoms a mass of protoplasm (πρωτος, first, and πλασμα, form) constitutes the simplest cell. The composition and function of protoplasm we will discuss presently; it suffices in the meantime to note that the lowest cell does not have a cell-wall distinct from the protoplasm. The cell composed of protoplasm bounded by a wall may be considered as occupying a step higher in the scale of organization. The generality of unicellular, or one-celled plants, however, are composed of such structures.

The size of the cell varies considerably. A bacterium cell may be so small as to require the highest powers of the microscope and the most accurate definition to determine its form; while it is still a matter of doubt if the branching tubes, filled with milky material, found in species of Euphorbia are not composed of a single cell. The hairs of many plants afford good instances of cells of large dimensions. Let anyone who cuts up an orange note the structure of the pulpy portion. A cursory examination will show him that it is built up of very large elongated cells, filled with a juicy material. These cells are the hairs of the inner wall of the fruit. And in no less a degree does the shape of the cell vary, as we shall see in our study of the tissues.

* Sachs' *Physiology of Plants*, page 73.

The truly vital portion of the plant and of the animal is the protoplasm. By its activity are all the parts of the body formed. The most complex plant and the most highly organized animal have had their origin in a small speck of protoplasm, and only so long as the protoplasm is alive can these be said to be living beings. The low power of a microscope reveals to us, in a drop of water taken from the bottom of a muddy pool, a small unicellular organism capable of locomotion and of alterating its form. It is known as an Amœba. Fig. 1 shows one of these animals in its various phases. The processes marked *ps* can be sent out or retracted at will, and are called pseudopodia (ψευδος, false, and πους, a foot), or false feet. By the protrusion and retraction of these processes of its body the animal moves from place to place, and movement of this kind is very common in the two organic kingdoms, and is termed amœbiform movement. More careful examination reveals the fact that the outer portion of the body is clear and the inner granular. The granular parts are probably portions of matter about to be formed into protoplasm, or which have resulted from its decomposition.

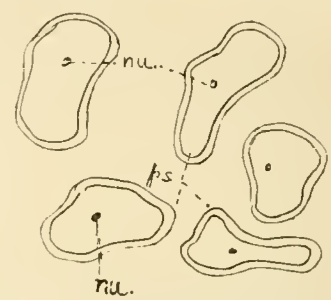


Fig. 1.—Diagrammatic representations of various phases assumed by an Amœba. The inner line indicates the boundary between the granular portion or endosarc, and the clear outer portion or ectosarc; *ps*, pseudopodia; *nu*, nucleus.

The mode of taking in food is also observed to be a very simple one. The animal encloses it by its false feet and absorbs it into itself; and its method of ridding itself of what it does not require, or cannot make use of for its nourishment, is no less complex—it ejects it at any spot on its surface. When it has grown large enough it breaks up into two pieces, and each portion so formed is a new Amœba, capable of performing the work which its parent previously did. All this can be seen very easily under the microscope. Amœbæ are not difficult to procure. For these reasons we have selected this animal to illustrate the work which a simple mass of protoplasm can perform. We see then that the phenomena of locomotion, nutrition (including ingestion, digestion of food, and ejection of waste products), and reproduction are all exhibited by this unicellular organism. To these we must add respiration, or breathing, the taking in of oxygen and giving out of carbon dioxide, which we know is also constantly going on during the lifetime of plants and animals.

In the higher plants the protoplasm is divided into pieces by walls, so that the whole organism consists of a number of chambers or cells. Each chamber in its young condition is filled with protoplasm; as they grow older the protoplasm in many cases disappears and leaves only the case in which it was contained, the cell-wall. The wall has not an origin apart from the protoplasm, but is deposited by the protoplasm itself. What we would

emphasize here is, that the protoplasm is the vital portion of the plant, and that from it all the parts of the plant are formed. Hence, in order to keep a plant alive, we must supply it with material containing the elements which enter into the constitution of protoplasm. Sachs speaks of "the universally known and yet essentially unknown protoplasm": it is *essentially* unknown, although, as the result of various chemical researches, its chemical composition has been roughly determined. Its chemical formula cannot be given, nor is this to be wondered at in the case of a body which is living, and hence probably constantly undergoing change. It is made up of proteids, complex compounds of oxygen, hydrogen, nitrogen, and sulphur, and when it is alive water is always present in it. If it be burned, these materials go off in the form of gas, but there remains behind an incombustible portion or ash, in which are present potassium, magnesium, calcium, phosphorus, and sulphur, in addition to other substances. All these elements—carbon, hydrogen, oxygen, nitrogen, sulphur, phosphorus, potassium, magnesium, and calcium—must be supplied in some form to a plant in order that it may live; for of these is its vital substance, or protoplasm, built up.

It remains for us now to determine in what manner these materials are taken in by a green plant, and what is their fate after absorption. The higher plants are those which are brought more immediately under our notice in our daily walks. We will, therefore, consider their method of feeding, premising, however, that it does not differ essentially from that of any green plant, however humble: but it will be useless to attempt a description of this until our readers have a clear notion of the structure of one of these. The remaining portion of this paper is thus devoted to an account of the anatomy of the elm tree—a typical dicotyledon.

It has a much branched root that it sends into the soil, and a stem branching in the air and bearing the leaves. The ultimate ramifications of the root are fine fibrils. These are its important portions. The tip of each rootlet is clothed with a cap (Fig. 2, *r.c.*), to prevent the growing tissue (*t*) situated immediately behind it from being damaged as the root pushes itself through the soil. As constantly as the outer layers of tissue are rubbed away, new ones, formed by the growing tissue, take their place. The root cap is thus being constantly renewed. At a slight distance behind the cap unicellular hairs are apparent. In a cross section of the root taken at this point it becomes evident that these are prolongations of some of the epidermal or outer skin cells (Fig. 3). The same figure shows the cortex or rind to be made up of iso-diametric cells containing protoplasm. The tissue they form is called parenchyma (παρα, and 'εγχυμα, a tube). Immediately beneath the cortex is the endodermis or bundle-sheath (ενδο, within, and δερμα, the skin) enclosing a zone of tissue composed of delicate cells, and called the pericycle. This in turn surrounds a mass of tissues forming the vascular bundle. The vascular bundle is a complex structure. It is composed of three chief portions—the xylem, phloëm, and cambium. The xylem (ξυλον, wood) or wood is made up of parenchyma, of elongated cells with walls spirally thickened and pitted (tracheides), and of vessels.

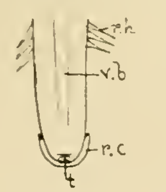


FIG. 2.—Diagrammatic longitudinal section of apex and front. *r.c.*, root cap; *t*, growing tissue; *r.b.*, vascular bundle; *r.h.*, root hairs.

Vessels are formed by the fusion of cells placed end to end, whose adjacent walls have broken down. The tracheides and vessels contain no protoplasm. The cambium is composed of thin-walled cells densely filled with protoplasm. Parenchyma and sieve-tubes are the con-

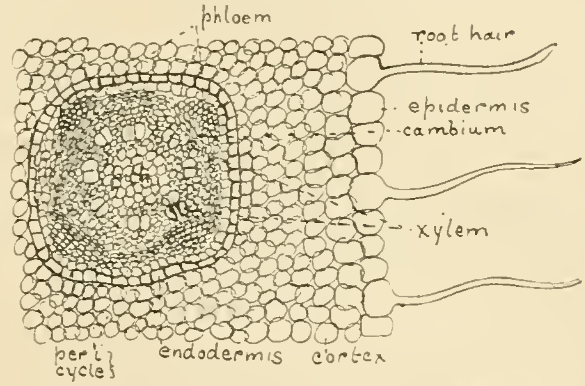


FIG. 3.—Transverse Section (semi-diagrammatic) of root.

stituents of the phloëm. The sieve-tubes have received their name from the fact that their adjacent walls are provided with apertures giving them the appearance of a sieve, and sieve-plates are also at times present on their lateral walls. The xylem and phloëm alternate in position with each other, and the cambium is placed between, lying outside the xylem and inside the phloëm. The central portion of the root is occupied by the pith, which is formed of parenchyma. From the medulla or pith proceed rays of tissue—the medullary rays.

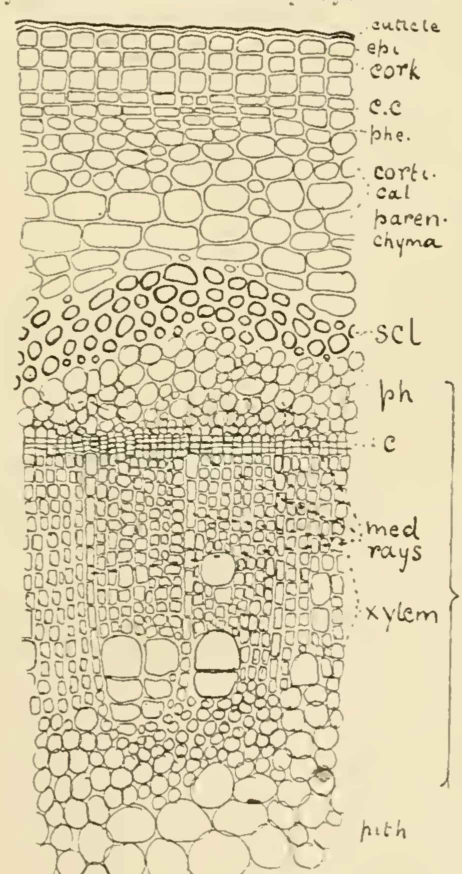


FIG. 4. Transverse Section of young stem of Elm. *epi*, epidermis; *c c*, cork cambium; *phe.*, phelloderm; *scl.*, sclerenchyma; *ph.*, phloëm; *c.*, cambium; *med. rays*, medullary rays; *v. b.*, vascular bundles. The cork cambium forms cork towards the outside, and phelloderm towards the inside. The phelloderm at this stage contains chlorophyll. The large openings in the xylem are wood-vessels.

Fig. 4 shows a cross section of a young elm stem. We see that outside there is an epidermis (*epi.*) whose outer walls are covered by a layer impervious to water, and called the cuticle : a number of vascular bundles form a zone about midway between the centre and the periphery. In the centre is the pith, and outside the bundle zone is the cortex. The vascular bundles are essentially the same in structure as those of the root. The cortex is made up of large cells, the outer of which are filled with chlorophyll or green colouring matter. Outside each vascular bundle is a patch of tissue formed of elongated hard-walled cells destitute of protoplasm, and called *sclerenchyma* (*scl.*) (σκληρος, hard). Uniting the pith to the cortex, and cut by the cambium, run the medullary rays (*med. rays*). The xylem or wood lies next the pith, and the phloëm (*ph.*) next the cortex. The phloëm is sometimes called the soft bast or inner bark. The stem of a cabbage, left exposed for some time to the action of the elements, does not decay uniformly. The cortex and pith disappear and leave a framework composed of the vascular bundles. These are continuous with the bundles of the root.

The leaves are expansions of the stem. The green leaves, with which we are specially concerned at present, are of very varying forms, but the *anatomy* of the foliage leaf of the elm and of any other dicotyledon is practically the same. A continuous layer of cells surrounds the whole structure. This is called the epidermis (Fig. 5, *ep.*); it is covered by a continuous corky layer called the cuticle, just as in the epidermis of the stem. Towards the upper surface there are two or three layers of parenchyma which generally appear like a palisade (Fig. 5 *p.p*), and have hence received the name of palisade parenchyma. Below that lies a loose parenchyma tissue, the mesophyll, Fig. 5, *me.* (μεσος, the middle, and φυλλον, a leaf), with many spaces between the cells—inter-cellular spaces—and towards the lower epidermis there is a layer of roundish cells. The continuous epidermis of the stem and leaf is broken by numerous minute apertures—stomata (στωμα, a mouth)—whose structure and function were described in a previous paper in Knowledge for November, 1890, on the "Breathing Organs of Plants." There it was stated that these little mouths are not breathing organs, but are the structures which regulate the giving off of water by the plant ; they are the *transpiring* organs.

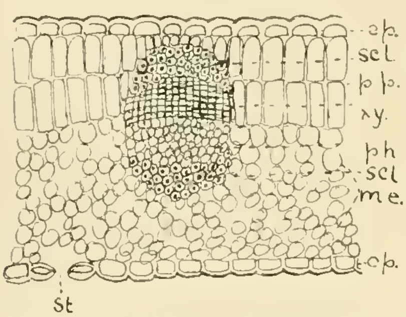


Fig. 5.—Diagrammatic transverse Section of Leaf. *ep.*, epidermis ; *scl.*, sclerenchyma ; *p.p.*, palisade parenchyma ; *xy.*, xylem ; *ph.*, phloëm ; *me.*, mesophyll ; *st.*, stoma. The black dot in the centre of the sclerenchyma cells represents the cavity of the cells.

A section of a leaf taken through a vein at once shows that this is merely a vascular bundle. Its course can easily be traced by dissection from the leaf blade, down the leaf stalk or petiole, to the bundles of the stem. The veins are the ultimate ramifications of these organs, but differ from them in not containing cambium.* The xylem is in consequence at the upper surface and the phloëm towards the under surface of the leaf blade. A sclerenchymatous sheath (*scl.*) more or less completely surrounds each bundle, adding strength to the structure. The mesophyll and palisade parenchyma are packed with chlorophyll granules. With the exception of the guard cells of the stomata there is no chlorophyll in the epidermis.

We close this paper by alluding to a few interesting facts in connection with the structure of this plant, which we will not have occasion to speak of afterwards. In the root we saw the pericycle, a layer of tissue which surrounds the vascular bundle. It has a very definite work to perform. When a young root is to be developed the cells of the pericycle situated opposite a xylem tract become active, divide up and give origin to the young root which breaks its way through the outer tissues of the parent root. The cambium of the vascular bundle of the stem and root becomes active in spring, and continues so during summer and early autumn ; its cells divide up to form on the outside phloëm and on the inside wood or xylem. In a two or three year old twig of the elm or other tree we find a mass of *wood* in the centre, and in the spring we can easily strip off the bark. The juicy portion which immediately surrounds the wood left is the ruptured cambium, whose delicate texture enabled the operation to be performed so easily. The wood increases much in excess of the phloëm. This can be seen by examining the stump of a recently felled tree, such as the elm or oak. The pith has not a long existence ; it soon becomes obliterated by the growth of the xylem. In the hemlock, and generally in the Umbelliferæ, the group of plants to which it belongs, the pith breaks down and the centre of the stem is occupied by a large cavity, interrupted at the nodes or parts where the leaves come off. The epidermis of a forest tree is soon shed, and its place is taken by a corky tissue (Fig. 4), formed from a layer of cells in proximity to the epidermis. This in turn is thrown off, and is replaced by a cork layer formed in a similar manner.

* In a monocotyledon, such as a palm or grass, the vascular bundles of the stem are not arranged in the manner stated, but are disposed irregularly ; moreover, they are *closed* bundles, that is, they possess no cambium.

SOME FACTS ABOUT EXPLOSIVES.

By Vaughan Cornish, B.Sc., F.C.S.

THE general phenomena of explosion are well known : noise, shock, resistance offered and overcome, and, in general, some work of destruction wrought, such as demolition of a structure or the disruption of a rock.

Various materials, solid and liquid, are employed for the purpose of producing and utilizing the effects of explosion. Every such material is termed an *explosive*, although sometimes, as in the case of gunpowder, the material is in fact a mixture of several substances ; coal gas and fire-damp are not called explosives, although on mixing with air they are capable of exploding. An explosive proper contains its own supply of oxygen.

Explosives are of two principal kinds, of which gunpowder and gun-cotton are typical examples. Gunpowder is a mixture of three different chemical substances, whereas gun-cotton (and similarly the other nitro-explosives, as, *e.g.*, nitro-glycerin) is a single chemical substance, composed, however, of several elements, one of these elements being oxygen.

In gunpowder we very intimately mix together two substances, charcoal and sulphur, which are capable of combining chemically with the oxygen contained in the third substance, nitre. All that is necessary to bring about this chemical change is, firstly, that the particles of the various substances shall be brought very close together, which is effected by the careful incorporation of the ingredients ; and, secondly, that the temperature should be high. At a

high temperature gunpowder takes fire, or, if the conditions be suitable, explodes. In the combustion of gunpowder the carbon burns to carbonic acid (a gas), the sulphur burns to sulphurous acid (a gas), and other gaseous products are also formed in considerable quantity.

The heat developed by the chemical reaction raises the temperature of the gases, which causes them to expand rapidly and occupy a large volume. If the burning of the gunpowder takes place in a confined space, the force of expansion of the gas is resisted by the walls of the enclosure, and if the latter are not of sufficient strength, the obstacles are overthrown. If, however, the enclosure be sufficiently strong and furnished with no outlet (as in certain experiments where very small charges are fired inside strong bombs), the gases formed are kept compressed in a small volume, and no disruption takes place.

Here we have the fact of explosion, without its usual striking accompaniments.

When a cartridge is fired in a gun-barrel, the gases, whose force of expansion would be sufficient to burst the steel of the barrel, find in one direction no great resistance, and expanding rapidly in this direction propel before them the bullet, which thus leaves the muzzle of the gun endowed with a high velocity.

Explosives are compounds or mixtures, which contain in themselves elements capable of taking up a new molecular arrangement forming fresh compounds, one or more of the new compounds being gases, and the formation of these compounds being accompanied by an evolution of heat. The formation of gases, and the development of heat in the reaction, are essential to the production of an explosion.

When we come to enquire what is the condition generally necessary to produce the explosion of an explosive body, we find that it is the rapid vibration of the particles. Such vibration may be generated by heat, by shock, or by friction, but in general the condition most favourable to explosion is one of rapid vibration, which may be produced by a sudden blow which will serve to detonate dynamite without appreciably heating it. The liability of the occurrence of explosion under such circumstances does not depend only on the *force* of the blow, but on the nature of the striking body with which the blow is given. Thus, a force of blow which would cause explosion if the blow were one of steel against steel might be harmless if produced by wood against wood. In this case the explosion is determined, not by the amount of heat produced, but by the rapidity of the vibration. It is well known that a tuning-fork struck against steel yields a higher note (*i.e.*, a sound of more rapid vibration) than if struck against wood.

When explosion occurs the chemical atoms are *shaken out* of one combination to fall into another. This process takes place much more readily when the explosive is warm.

Although explosions are often produced independently of heating effects, yet it must be borne in mind that at a sufficiently high temperature all explosives will detonate.

In the study of phenomena connected with explosives we often meet with occurrences at variance with the old dog-Latin dogma, *causa æquat effectum*. The pulling of the trigger, followed by the rush of the bullet from the gun, is a familiar example; the work done in pulling bears no proportion to the energy developed by the explosion. Another case in point is afforded by the manner in which explosions are sometimes caused in the incorporating mills, in which the component materials of gunpowder are mixed together. The presence of a small hard body, such as a nail, or even a hard piece of grit, may cause sufficient local heating to start an explosion of the whole mass. Both shocks and local heating are most carefully guarded against in gunpowder factories. Charcoal possesses a property (that of condensing air in its pores) which sometimes leads to local heating and induces spontaneous combustion.

In grinding the sulphur there is another source of danger. Sulphur is a highly electric body, and in the process of grinding a large amount of electricity will often accumulate, sometimes giving rise to sparks, the passing of which may produce serious consequences. The danger from this source is, however, to a great extent overcome by connecting the sulphur mills to earth by means of copper wires, and thus continually drawing off the charge of electricity produced by friction in the grinding process.

In the pressing of the gunpowder, hydraulic machines furnished with ebonite plates are frequently employed. Ebonite is a convenient material for the purpose, being tough, elastic, smooth, and sufficiently hard. Unfortunately, ebonite is a highly electric material, and the upper and lower plate, with the cake of powder between them, form practically an electric pile. A passing thunderstorm may induce a discharge of sparks from the ebonite, igniting the gunpowder and producing, as has happened in several cases, fatal accidents.

In spite of all precautions, explosions are liable to occur in the mixing of the materials for gunpowder, and it is well to provide, as far as possible, for the safety of the *employés* and of the building. A good protector for the workmen is a curtain of ships' hawsers, which offer the kind of resistance which is most effectual in the case of an explosive outburst of gas.

By having a light roof, secured only by one or two wooden pins, an outlet is obtained for the gases produced in a factory explosion. The roof is simply lifted off, and the outlet thus given prevents the pressure inside the building from becoming sufficiently great to damage seriously the main portions of the building.

Turning now from the explosive *mixture* gunpowder to the nitro *compounds*, such as gun-cotton and nitro-glycerin, we find that the nitro bodies explode more readily under shock, and also at a lower temperature. Nevertheless, in the hands of properly-trained workmen, the manufacture of nitro-glycerin and dynamite is accompanied by fewer casualties than that of gunpowder. Gun-cotton is prepared by the action of strong nitric acid and sulphuric acid upon cotton-wool. Most of the processes are carried out in presence of a large excess of water, though this is, of course, not the case during compression, in which operation great care has to be exercised.

Nitro-glycerin, prepared by acting upon glycerin with a mixture of nitric and sulphuric acids, is liable to explode both by heat and by shock.

Dynamite is produced by absorbing three parts of nitro-glycerin by one part of kiesulguhr, a finely-divided siliceous earth capable of absorbing a large quantity of liquid without becoming pasty. Dynamite only explodes when subjected to special treatment, being unaffected by moderate heat or by an ordinary blow, but detonating under the sharp shock given by a percussion fuse of fulminating mercury. The kiesulguhr plays no part in the actual explosion, so that dynamite, as an explosive, must be classed along with gun-cotton and the other *compounds*, rather than with explosive *mixtures* such as gunpowder.

The smokeless gunpowders now coming into general use are prepared from gun-cotton, or from gun-cotton and nitro-glycerin.

A special class of explosives are required for filling percussion caps and detonators. Fulminate of mercury is the most important of these highly dangerous substances, the

manufacture of which is conducted with the most elaborate precautions, not only against shock, but against the smallest amount of friction.

The protection of factories against lightning is a problem of considerable difficulty. According to Mr. Otto Guttmann, whose recent paper before the Society of Chemical Industry contains much useful information on this and other matters connected with the dangers of explosives, a system similar to that of Professor Lodge's "network" protector has been extensively and successfully used by Austrian military authorities. The system is similar to that by which electrometers are shielded from electrification by means of a wire cage, the building being covered by a network of galvanized iron wire. This material is, of course, much cheaper than copper, and its smaller electric conductivity does not appear to be a serious drawback in the case of electric discharges of such high potential as that of lightning.

RADIOMETRY.

By A. Jameson.

The kinetic or, as it is sometimes called, the molecular theory of matter, by which its sensible qualities are referred to the motion of atomic and molecular parts, and the undulatory theory of light, which asserts that radiation is due to transverse waves in a medium with which all space is filled, have become thoroughly incorporated with modern physical science. Employed in the first place as working hypotheses, these theories have gradually become established as truths. To give anything approaching a complete exposition of them would be a most extensive undertaking. But, without pretending to this, it is thought that a discussion of some few typical instances in which the operations of these laws have been recognised may prove interesting.

Prof. Crookes' radiometer, or light mill, furnishes a remarkable example of the conversion of energy in the form of ethereal waves into molecular and subsequently molar motion. The commonest form of radiometer is shown in Fig. 1. Four light vanes of mica, attached to radial wire arms, are fixed to a central cup, that is balanced, like that of a compass needle, upon a fine steel point. One side of each vane or paddle is painted a dull black with lampblack; and it is upon these lampblacked faces of the vanes that molecular pressure, resulting from radiation, will be exerted. A small glass tube is fixed vertically over the central pivot cup. In the position shown this tube is free from contact with the fly, but serves, should the instrument be inverted, to prevent it from toppling off the needle-point. The glass bulb, having been highly exhausted by a mercury pump, is hermetically sealed.

Fig. 1.

Remembering that what we call heat is simply a state of vibration of the molecular parts of bodies—restricted in the case of solids and liquids, extending to perfectly free excursions in the case of gases—we will proceed to consider the effect of radiation, as, for instance, of diffused daylight, upon the piece of apparatus just described. Light traverses the glass envelope freely, because, as we may assume, there is no correspondence between the periods of the luminous waves and those of the molecular vibrations in glass. With the lampblacked surfaces of the vanes, however, the case is very different. Here the periods of the vibrations accord so nearly with those of waves of light that complete absorption takes place before an appreciable thickness of the substance has been penetrated. Just as the swing of a pendulum is amplified by properly timed impulses, so the swing of a molecule responds to transverse ether waves of suitable frequency; and light waves are quenched by the lampblack in consequence of the conversion of their energy into molecular motion, that is to say, heat. Mica, like glass, and like most gases, is a transparent substance, whence it follows that light falling upon the clear surfaces of the vanes will also be transmitted to the layers of lampblack, and by them absorbed, with corresponding elevation of their temperatures. So little absorption takes place in mica that practically all light falling on the bright sides of the vanes is either transmitted as described or is reflected. But, in some instruments, the mica surfaces are coated with a bright metallic film, and in such cases, excepting only at the blackened parts, nearly all of the incident light will be reflected. Where light is not absorbed, of course it cannot be the source of heat; and therefore no increase of temperature, corresponding to that which takes place in the lampblack, can occur at the polished surfaces of the vanes. Undoubtedly some heat will be *conducted* through the mica from the blackened to the polished side; but since this material is an extremely bad conductor, the amount of heat thus transmitted must be small. On the other hand, heat will pass rapidly from the warmed lampblack to the air or other gas with which it may be in contact; and it will presently be shown to be this kind of transference of heat that accounts for the rotation of the light mill.

To make this matter clear we will suppose, in the first place, that the glass bulb contains gas at the ordinary pressure of the atmosphere, and we will confine our attention to one only of the four similar and similarly situated mica vanes. As the molecular vibrations at the surface of a solid body, such as lampblack, will take place with most freedom when normal to that surface, we may expect every gas molecule that strikes the warmed face of the vane to be thrown back in some direction, making a smaller angle with the perpendicular than does the direction of incidence. Thus the lampblack will give rise to a "molecular wind," made up of gas molecules whose paths are, upon the average, nearly perpendicular to the heated surface. But the mean free path of gaseous molecules at the normal temperature and pressure is extremely small. It is estimated by Maxwell, in the case of hydrogen, at 965 meter tenths (say $\frac{1}{10000}$ mm.), in the case of oxygen and of carbonic acid at 560 and 430 meter tenths* respectively. Hence the molecules rebounding from the heated surface will encounter others, and will thereby have the direction of their motion altered many times before they travel an appreciable distance; and hence the extra speed acquired by the gas molecules by collision with the hot lampblack—the extra temperature, in other words, of these free molecules—will be diffused in every direction through the atmosphere within the bulb. Now, since the blackened surface of our vane is bombarded by molecules having an average velocity corresponding to (say) 15° (or whatever may be the initial temperature of the instrument) and since these molecules are thrown off again with an average velocity corresponding to (say) 15·1°, it might be thought that molecular pressure should be developed, even in the circumstance we have supposed, that is, in gas at the normal pressure of the atmosphere. For action and reaction must be equal and opposite; and

* 1 m. tenth = 1 m. × 10^{-10}

provided the same number of molecules per second struck the warm and the cold face of the vane, it is certain that the additional work of repulsion done upon them at the former surface would react upon the vane itself, and drive it in the opposite direction from the shower of heated molecules. As a matter of fact, however, this does *not* take place; for the density of the gas in that thin heated layer, contiguous to the lampblacked surface, is less in proportion as its temperature is higher than in any other part of the bulb. The quickened molecules, shooting away from the lampblack, beat back a proportion of those that move more slowly; and thus the number of impacts on the heated surface is reduced, and the equilibrium of the two sides of the vane is almost perfectly maintained.

Next consider the effect of exhausting the glass bulb of the radiometer. The mean free path of the gas molecules will increase directly with the exhaustion, while the average distance from centre to centre of the molecules will only increase with its cube root. Hence, by a sufficient degree of rarefaction, the free path can be made quite considerable, though the molecules are still crowded together in enormous number. At an exhaustion of one-millionth of an atmosphere, the free path will be a million times augmented—that is, to a length of about ten centimetres—and it is under pressures ranging from this point up to about fifty-millionths of that of the atmosphere that light mills exhibit the highest efficiency. As exhaustion of the bulb is carried out, new conditions of cooling begin to manifest themselves before the heated lampblack. As the free path increases, the "molecular wind" that has been spoken of blows further, and cooler particles of gas crowd in upon the blackened surface from the sides. By-and-by the shower of molecules (moving at a velocity corresponding to 15·1°, and in a direction normal to the surface of the vane) will extend to the glass envelope, and will communicate their heat to it directly. Comparatively few will now collide with the colder molecules travelling in the opposite direction; and these colder molecules, no longer unduly checked in their journeys to the blackened surface, encounter it as often as they do the other side. The *reaction*, or Crookes' pressure, at the warmed surface will therefore assert itself, and occasion the rotation of the fly.

When first the radiometer was exhibited, it was suggested that the sunny side of the earth would suffer a force of repulsion comparable with that upon the vane of such an instrument. It is almost unnecessary to say, however, that there is no foundation for this idea in the now accepted explanation of Prof. Crookes' discovery. Crookes' pressure is only indirectly due to radiation, and it is quite distinct from that estimated by Maxwell (see his *Electricity and Magnetism*, vol. ii., p. 402) at about 2½ lbs. per square mile of the sunlit surface of the earth.

FIG. 2.

Prof. Crookes' mechanism is applicable to the conversion of radiant energy, of probably almost any wave length, into a form convenient for approximate measurement. But, from the definition that has been given of absorption, it follows that the layers of lampblack on the vanes may have to be replaced by various other materials, according to the quality of the radiation that it is desired to measure. Radiometers sensitive to actinism and to radiant heat have been constructed, and it may be presumed that such instruments can also be made to respond to the electrical radiations that have been studied by Prof. Lodge and others.

The constructive details of the apparatus can be modified in very many ways. For instance, a fly having several plain mica vanes, skewed like the blades of a screw propellor, may be placed in the path of the shower of molecules proceeding from a fixed disc of lampblacked material. In consequence of the favourable situation of the reacting surfaces, very rapid rotation takes place in this modification of the radiometer. It has been named by Prof. Crookes the otheoscope, and is shown in Fig. 2. If the skewed vanes of the otheoscope could be rotated sufficiently rapidly by mechanical means, a stream of molecules would be generated, not unlike that which is set up by the heating of the lampblacked disc. The study of molecular motion under such conditions is not without practical interest. Indeed, with every different point of view from which the radiometer can be regarded, some attractive suggestion presents itself.

Chess Column.

By C. D. LOCOCK, B.A.Oxon.

ALL COMMUNICATIONS for this column should be addressed to the "CHESS EDITOR, *Knowledge Office*," and posted before the 10th of each month.

Solution of June Problem (by C. D. Locock).—1. Q to R5, and mates next move.

CORRECT SOLUTIONS received from Alpha and C. T. Blanshard.

H. S. Brandreth.—If 1. Q to B5, Black replies P × R, or Kt to K6, and no mate results.

J. C. Knocker.—There is a flaw in your conclusion. After 1. Q × P, Black provides an escape for his King by Kt to K6.

C. Leeson Prince.—After 1. Q × Ktch there is no mate.

Alpha.—"Dummy Pawns,' as they are called, are no longer allowed. There is also a prejudice against any promotion on the first move, just as there is against checks and captures.

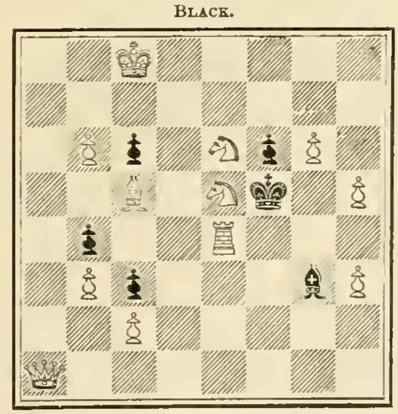
C. T. Blanshard.—Thanks for the Problems. The promotion problem is a little too simple, apart from the fact that RP becomes a Queen is equally effective. The other seems all right. The three-mover is neatly solved in one move. In the variation you give the Rook and not the Bishop mates. Perhaps the diagram was wrong.

PROBLEM.

By J. JUCHLY, Munich.

(From the *Field.*)

BLACK.



WHITE.

White mates in three moves.

We fully endorse a statement in the *Hackney Mercury* as to the difficulty of the above. The chief difficulty lies

in finding defences against other key-moves than the correct one.

Consultation Game played at the Boston Chess Club, on the 8th April, by W. Steinitz against Messrs. J. F. Barry, C. B. Snow, and H. N. Pillsbury. The score is from the *Liverpool Mercury*.

[KING'S GAMBIT DECLINED.]

Steinitz.	Allies.
1. P to K4	1. P to K4
2. P to KB4	2. B to B4
3. KKt to B3	3. P to Q3
4. B to B4 (*a*)	4. QKt to B3
5. P to B3	5. Kt to B3
6. Q to K2	6. Q to K2 (*b*)
7. P to Q3	7. B to KKt5
8. P to B5 (?)	8. Castles, QR
9. P to Kt4 (*c*)	9. B×Kt
10. P×B (*d*)	10. P to Q4
11. P×P (*e*)	11. Kt×QKtP (*f*)
12. P to Q4	12. B to Kt3
13. B to R3	13. KKt×P
14. Q to K4 (*g*)	14. Kt×QBP!
15. Kt×Kt	15. R×P
16. Q to K2	16. Q to R5ch
17. K to Bsq	17. R×B
18. B×Kt (*h*)	18. R×B
19. Kt to K4	19. R to Qsq
20. K to Kt2 (*i*)	20. R×Kt!
21. P×R	21. R to Q7 (*j*)
22. Q×R	22. Q to Kt5ch
23. K to Bsq	23. Q to B6ch
24. K to Ksq	24. Q×Rch
25. K to K2	25. Q×R
26. Resigns.	

NOTES.

(*a*) 4. Kt to B3 is at least equally good. 4. P to B3 may also be played.

(*b*) With a view to castling on the Queen's side if desirable.

(*c*) Premature, as Black's excellent play demonstrates; but in any case his centre must be broken up by P to Q4.

(*d*) If 10. Q×B, Kt×KtP; 11. P×Kt, B to Q5, &c.

(*e*) Messrs. Pillsbury and Barry, in their notes to this game, suggest as an alternative 11. B to Kt3, Kt×KtP; 12. P×Kt, B to Q5; 13. B to Kt2, Q×P ch.; K to Bsq, Kt to R4; with a strong attack. The position after these moves is most remarkable, White having scarcely a move which he can make without loss. Perhaps, therefore, his safest course at move 11, was simply 11. P×B, P×B; 12. P×P, Q×P; 13. B to K3, Q to R4; 14. Castles, with fair prospects of ultimate safety.

(*f*) A very ingenious sacrifice. If the Knight be taken, Black clearly wins back the piece by 12. . . . B to Q5.

(*g*) Overlooking Black's crushing reply. But even after 14. B×KKt, Q to R5ch; 15. K to Bsq, Kt×B, he would have a very bad game.

(*h*) If 18. Kt to K4 at once. Black wins by 18 . . . R to B7, for the Knight must cover.

(*i*) Black threatened to win a piece by R×Kt and Q to R6ch, &c. 20. Kt to Kt3 is useless on account of 20 . . . Q to Q5 and 21. . . . R to Kt7.

(*j*) A beautiful final coup which wins a clear Rook. Black's play throughout against their formidable opponent has been of the very highest order. The innovation of castling on the Queen's side turned out most successfully.

CHESS INTELLIGENCE.

The American Championship Match was concluded last month, the final score being Lipschütz, 7; Showalter, 1; drawn, 7. Mr. Showalter is well known as a rising, and at times a brilliant player, but in the present match the brilliancy seems to have deserted him. Nor are his previous performances in any way to be compared with those of his opponent, who is evidently in America second only to Mr. Steinitz. The large proportion of drawn games is noticeable.

The *Hackney Mercury* announces an Autumn Problem Tourney. Entries to be sent in by September 1st. Under certain conditions corrected versions of previously published unsound problems will be admitted. There are two sections (for two-move and three-move direct mates respectively), and four prizes in each section. Composers are limited to one problem in each section.

The Divan Handicap has again been won by Mr. F. J. Lee, with the good score of 12½ out of a possible 16. Mr. Mollard (Hon. Sec. of the South Norwood Chess Club) was second, with 11½. Mr. Loman, who made such a fine score against the rest of the first class that his ultimate victory was looked on as assured, failed signally as a giver of large odds; his last game gave Dr. Alderson his solitary victim.

The long-expected match at the British Chess Club, between Messrs. Blackburne and Lasker, ended in a decisive victory for the young German master by 6 games to 0, with 4 games drawn. This performance, coming as it does after his two previous tournament successes, seems to settle the Championship of England question probably for some time to come. Possibly Mr. Blackburne was not at his best throughout, notwithstanding his splendid combination in the middle of the ninth game. Mr. Lasker's play is not always intelligible to the general public; in the words of Mr. Bird, he is "a very mysterious player." Perhaps the mystery is partly explained by the fact that when he sees nothing to do he is content to do it. Bold and unexpected Pawn-play is his speciality. Probably altogether he has no equal in Europe except Dr. Tarrasch.

CONTENTS OF No. 80.

TERMS OF SUBSCRIPTION.

"KNOWLEDGE" as a Monthly Magazine cannot be registered as a Newspaper for transmission abroad. The Terms of Subscription per annum are therefore as follows:—To any address in the United Kingdom, the Continent, Canada, United States, Egypt, India, and other places in the Postal Union, the Subscription is 6 shillings, including postage; or 1 dollar 50 cents; or 6 marks; or 7 francs 50 cents.

For all places outside the Postal Union, 5 shillings in addition to the postage.

Communications for the Editor and Books for Review should be addressed, Editor, "KNOWLEDGE" Office, 326, High Holborn, W.C.

KNOWLEDGE
AN ILLUSTRATED
MAGAZINE OF SCIENCE
SIMPLY WORDED—EXACTLY DESCRIBED

LONDON: AUGUST 1, 1892.

CONTENTS.

THE LIQUEFACTION OF GASES.

By Vaughan Cornish, M.Sc., F.C.S.

The common liquids, such as water, rock oil, mercury, and so on, can be readily converted into gases; but many of the common gases, on the other hand, for instance oxygen, nitrogen, and hydrogen, can only be brought into the liquid condition by the use of special methods and powerful agencies. Temperature and pressure are the two factors on which it depends whether a body remains in the state of gas or assumes the liquid condition. Low temperature and high pressure are the conditions favourable to liquefaction.

The history of experiments on liquefaction of gases is mainly a record of devices for producing high pressure and low temperature. Sulphurous acid gas is condensed at the temperature of an ordinary freezing mixture, or at the pressure which can be obtained by a hand-worked piston in a tube or barrel. It had been prepared in the liquid state before the year 1800 A.D. Chlorine was condensed by Northmore in 1805, but his experiments attracted little attention till years later, when Faraday had made a speciality of the liquefaction of gases, and the attention of the scientific world was drawn to the subject. Then, as usually happens, forgotten records were found of earlier work on the same lines. The later, but independent, observation by Faraday (1823) of the liquefaction of chlorine is, however, the commencement of the systematic study of the subject. Faraday has the only kind of priority which is of real importance in scientific discovery—that, namely, of being the first to make the subject fruitful, and the first to make its importance generally understood. Faraday had been experimenting on the solid hydrate of chlorine which separates out in yellowish crystals when ice-cold water is saturated with chlorine gas. Sir Humphry Davy, to whom at that time Faraday acted as assistant, suggested that the crystals should be sealed up in a glass tube and heated. Davy gave at the time no reason for his suggestion, and Faraday himself did not know what to anticipate from the experiment. The crystals of the solid hydrate were placed at one end of a Λ-shaped glass tube, which was then closed by sealing up the glass in the blow-pipe flame. The crystals being warmed to 60° F., underwent no change, but at 100° F. "the substance fused, the tube became filled with a bright yellow atmosphere, and on examination was found to contain two fluid substances; the one (chlorine water), about three-fourths of the whole, was of a faint yellowish colour, having very much the appearance of water; the remaining fourth was a heavy bright yellow fluid, lying at the bottom of the former without any apparent tendency to mix with it." At 70° F. the pale portion congealed (*i.e.*, the hydrate separated out), although even at 32° F. the yellow portion did not solidify.

Heated up to 100° F., the yellow fluid appeared to boil, and again produced the bright coloured atmosphere. It was found that by heating to 100° F. the yellow liquid (fluid chlorine) could be distilled from the pale coloured liquid (chlorine water) so as to get them in different limbs of the bent tube. "If, when the fluids were separated, the tube was cut in the middle the parts flew asunder as if with an explosion, the whole of the yellow portion disappeared, and there was a powerful atmosphere of chlorine produced; the pale portion, on the contrary, remained, and when examined proved to be a weak solution of chlorine in water with a little muriatic acid."

The paper from which the above extracts are taken was read before the Royal Society by Sir Humphry Davy in 1823. In a note at the end of Faraday's paper, Davy says that "in desiring Mr. Faraday to expose the hydrate of chlorine to heat in a closed glass tube, it occurred to me that one of three things would happen: that it would become fluid as a hydrate; or that a decomposition of water would occur (forming hydrochloric acid); or that the chlorine would separate in a condensed state." Further on he remarks, "I cannot conclude this note without observing that the generation of elastic substances in close vessels, either with or without heat, offers much more powerful means of approximating molecules than those dependent upon the application of cold, whether natural or artificial, for as gases diminish only about $\frac{1}{480}$ in volume for every degree of Fahrenheit's scale, beginning at ordinary temperatures, a very slight condensation only can be produced by the most powerful freezing mixtures, not half as much as would result from the application of a strong flame to one part of a glass tube, the other part being of ordinary temperature, and when attempts are made to condense gases into fluids by sudden mechanical compression, the heat, instantly generated, presents a formidable obstacle to the success of the experiment, whereas in the compression resulting from their slow generation in close vessels, if the process be conducted with common precautions, there is no source of difficulty or danger, and it may easily be assisted by artificial cold, in cases where gases approach near to that point of compression and temperature at which they become vapours."

This "bent tube" method was successfully employed by Faraday in the liquefaction of a number of other gases.

In 1822, the year preceding Faraday's experiments on chlorine, Caignier de la Tour had examined the effects

produced by heating volatile liquids, such as alcohol or ether in closed tubes. In these experiments the liquid put into the tube was sufficient to fill it half-full, and the whole of the tube was heated; so that the conditions were different from those of Faraday's experiments. De la Tour observed that up to a certain temperature the liquid continued slowly to evaporate, the bulk of liquid diminishing, and the quantity of vapour increasing. This was the ordinary process of evaporation, which the eye can trace by observing that the position of the *meniscus* which separates the liquid and the vapour becomes lower and lower as the temperature increases.

When, however, a certain temperature is reached the *meniscus* suddenly disappears, and there no longer appears to be any liquid in the tube. On cooling the tube, the inverse change takes place with equal suddenness, a *meniscus* (the line of separation between liquid and vapour) suddenly appears, showing that on lowering the temperature a large amount of liquid is suddenly formed. De la Tour's experiments point to conclusions quite opposite to those of Davy, quoted above; since the "approximation of particles" brought about by pressure was more than counterbalanced by the repellant action of heat.

In 1845, in a second paper on Liquefaction of Gases, Faraday writes with a mastery of the subject which neither he nor Davy possessed in 1826 He says (*Phil. Trans.*, 1845, p. 155): "My hopes of success beyond that heretofore obtained depending more upon depression of temperature than on the pressure which I could employ in these tubes, I endeavoured to obtain a still greater degree of cold. There are, in fact, some results no pressure may be able to effect. Thus, solidification has not yet been conferred on a fluid (*i.e.*, a gas) by any degree of pressure. Again, that beautiful condition which Caignier de la Tour made known, and which comes on with liquids at a certain heat, may have its point of temperature for some of the bodies to be experimented with, as oxygen, hydrogen, nitrogen, &c., below that belonging to the bath of carbonic acid and ether, and in that case no pressure which any apparatus could bear would be able to bring them into the liquid or the solid state." The "bath of carbonic and ether" here referred to, was a device for obtaining a very low temperature, a kind of improved freezing mixture in fact. Thilorier and afterwards Natterer had constructed apparatus in which carbonic acid gas could be liquefied by pressure alone. When liquid carbonic acid is exposed to the air at the ordinary pressure, some of the liquid evaporates very rapidly, thereby chilling the lower layers of liquid to such an extent that they freeze, forming solid carbonic acid. If ether be mixed with this solid carbonic acid, and if the pressure be diminished by means of the air pump, the further cooling due to evaporation of the ether reduces the temperature to $-110°$ C. This extremely low temperature was employed in experiments by which Faraday endeavoured unsuccessfully to liquefy hydrogen, oxygen and nitrogen. Andrews employed the same means of producing cold, but used more powerful apparatus for compression. Although by combined cold and pressure the above-named gases were reduced to less than $\frac{1}{500}$ of their original volume no liquefaction took place. Andrews also conducted experiments upon the phenomenon observed by De la Tour, and showed that above the "critical temperature" of 31° C. the greatest pressure which he could bring to bear was not sufficient to liquefy carbonic acid, but that if the temperature were lowered the liquefaction took place at once.

In the later and successful experiments on the liquefaction of the "permanent" gases, such as nitrogen, oxygen, and hydrogen, the skill of the experimenter has been chiefly shown in the means devised for obtaining temperatures below the "critical point" of these gases. Pictet, of Geneva, who liquefied oxygen in 1877, relied for the production of a low temperature upon the well-known principle of latent heat of evaporation. The novelty in his application of this principle lay in the fact that he employed *two* evaporating substances—namely, sulphur-dioxide and carbonic acid. By the use of the doubly-acting pumps employed in refrigerating machinery (*vide* Knowledge, March, 1891, "Artificial Cold") liquid sulphur-dioxide can be obtained having a temperature of $-65°$ C. In Pictet's apparatus the cold liquid sulphur-dioxide is contained in an annular vessel, which forms a jacket round the tube in which carbonic acid is condensed by the action of another doubly-acting pump.

The liquid carbonic acid is reduced to a temperature of 65° C. by the cooling action of the "jacket," and, consequently, when the pump is reversed and used as an exhaust pump, the evaporation of the cooled liquid produces an extremely low temperature, and the vapour can again be condensed so as to form a liquid jacket round the oxygen tube, having a temperature of $-130°$ C. This tube of copper was made very strong to withstand pressure. The oxygen gas was passed into the tube direct from a strong iron retort in which it was evolved, and thus the pressure continued to rise as more and more oxygen entered the tube. At a pressure of about 500 atmospheres, the manometer remained stationary, showing that liquefaction had begun. The whole of the tube was at length filled with liquid oxygen, which was examined by opening a stopcock, when a jet of a lustrous liquid issued with great force from the tube, to be speedily dissipated by evaporation.

Cailletet, who worked independently at the same problem, first succeeded in liquefying oxygen on the very same day as Pictet. He employed simpler appliances, and worked on a different principle. He relied for obtaining his frigorific effect upon the *chaleur de détente*, or latent heat of expansion, of the gas with which he was working. The term "latent heat of expansion" is not a very good one, as gas is not cooled by merely expanding to fill a vacuum. When, however, a gas is allowed to expand in such a way as to do mechanical work, the gas loses in heat the thermal equivalent of the mechanical work performed. In Cailletet's experiments, the gas was contained over mercury in the capillary bore of an immensely strong glass tube. The tube was screwed into an hydraulic press worked by the leverage of a large wheel. The experimental glass tube was surrounded by a freezing mixture of which the temperature for the experiments was not lower than about $-30°$ C. So that in Cailletet's experiments no attempt was made to surround the gas with a very cold atmosphere. When the pressure attained 300 atmospheres the oxygen still remained in the gaseous condition, being much above the critical temperature, but, on suddenly withdrawing the constraining force from the piston of the press, the gas as suddenly expands, the elasticity or spring of the gas drives back the liquid and the piston, and the sudden mechanical effort of the gas is accompanied by a sudden chill sufficient to bring the temperature below the critical point. The liquefied oxygen was seen in the tube immediately the pressure was released. Cailletet's method had the advantage, that the process could be watched through the glass walls of the tube. On the other hand, Pictet's arrangement enabled him to prepare a larger quantity of the material, and to observe its behaviour when exposed freely to the atmosphere. As we have said, the liquefied oxygen was dissipated immediately by evaporation, so that no examination of its properties could be made. A fine mist, or

cloud, which formed when the oxygen evaporated may have been due to small particles of solid oxygen.

Experiments conducted since those of Cailletet and Pictet, fifteen years ago, have been designed so as to permit of the examination of the physical properties of the liquefied substances. This has been effected by obtaining such low temperatures of the material, and such a low temperature of its immediate surroundings, that the evaporation of the liquid (whether hydrogen, oxygen, or nitrogen) only takes place slowly under ordinary atmospheric pressure. In Cailletet's experiments, a liquid was obtained under low pressure, but the surroundings of the liquid were relatively warm, so that the substance could not long remain liquid. In Pictet's experiments the liquid could only be examined by removing it from the cold atmosphere. In the experiments conducted by Dr. K. Olszewski, the gas to be experimented on was contained in the innermost of four glass tubes placed one within the other. In the outermost tube was placed solid carbonic acid and ether. By placing this in connection with an air pump, the temperature of the neighbouring tubes was reduced to $-100°$ C. This was the method for obtaining low temperatures, employed by Faraday in his later researches. Now comes a novelty. Ethylene gas, brought from a Natterer's cylinder, is led into the (second) inner tube. Here it is liquefied by low temperature, and under a considerable pressure. The two innermost tubes (into which the oxygen or hydrogen are presently to be brought) are now surrounded by a tube containing liquid ethylene at about $-100°$ C. This liquid is protected from the warmth of the air by the outermost jacketing tube of carbonic acid ice. By the action of an air pump the pressure on the liquid ethylene is reduced to 10 mm. of mercury (about $\frac{1}{76}$th the ordinary atmospheric pressure); dry air at the same time is cautiously blown through the ethylene to prevent its evaporation from becoming violent. The gas (say oxygen) is now passed into the two innermost tubes. The intense cold produced by the evaporation of the liquid ethylene, the liquid being to begin with at about $-100°$ C., liquefies the oxygen, which is under a considerable pressure. But one more device remains to be mentioned, the most singular of all in Dr. Olszewski's process. The *two* innermost tubes, as has been said, contain the liquefied oxygen. They are now both put into connection with the air pump, and the pressure is cautiously diminished. The liquid in both tubes begins to evaporate and is thereby chilled. Presently, the liquid in the outer tube begins to evaporate more quickly than that in the innermost one, owing to the fact that it is in contact with the (relatively) warm ethylene tube. The whole of the liquid in the outer tube consequently evaporates whilst there still remains a considerable portion of the liquid in the innermost tube. The temperature of the innermost tube has now sunk considerably lower than that of the liquid ethylene. Nitrogen can be frozen in this way. By diminishing the pressure on the solid nitrogen, and thereby causing evaporation, Olszewski obtained a temperature of $-225°$ C., or less than 50° C. from the supposed absolute zero of temperature, that is to say, the point at which *all* the heat has been extracted from a body.

The exceedingly cold liquid contained in the innermost tube is protected from the relatively warm ethylene by the non-conducting layer of rarefied gas in the intermediate tube. Consequently, the substance remains liquid at atmospheric pressure, or even at lower pressure for a space of time (five to fifteen minutes) sufficient to allow of an examination of some of the important physical properties. Thus the specific gravity is determined by measuring the height at which the liquid stands in the tube, hence deducing the volume of the liquid; then collecting the gas after evaporation, and measuring its volume. The weight of a given volume of *gas* is, of course, well known. This is necessarily equal to the weight of the liquid before evaporation. Hence we know the weight of the liquid in the tube. Its volume having been ascertained in the way described, the specific gravity is readily calculated under atmospheric pressure; it is found that—

	Melts at.	Boils at.	Critical Temp.
Oxygen ...	—	$-164°$	$-118·8°$ C.
			Sp. gr. of liquid. 1·124 at $-181·4°$ C.
Nitrogen ...	$-214°$	$-194·4°$	$-146°$ C.
			Sp. gr. of liquid. ·885 at $-194°$ C.

Hydrogen at $-213°$ C. liquefied under a pressure of 190 atmospheres; with Pictet's temperature of 140° C. a pressure of 650 atmospheres was required. The temperatures recorded in the above observations were all registered with a hydrogen thermometer. The critical point of hydrogen is $-220°$ C.

BEE PARASITES.—II.

By E. A. Butler.

(*Continued from page* 128.)

The economy of the solitary bees is, of course, quite unlike that of the social species, and it will be anticipated that the parasitism from which they suffer is also of a different type. In solitary bees the species consist only of males and females, and hence all the labour necessary for the rearing of the young falls upon the latter sex. They excavate tunnels in sandy or clayey banks, posts, trees, or wherever else the habits of the particular species dictate, and in these burrows cells are arranged, each intended for a single grub; the eggs are deposited upon a previously provided stock of food, consisting chiefly of the pollen of flowers, often made up with a little honey into small pellets. As the larva, like that of the hive bee, is a footless maggot, quite unable to leave the cell in which it is cradled, and is not continuously tended by its parent, the amount of food provided must be such as will last it during the whole of its larval life. This quantity is not large, considerably less in fact than might have been expected, but its amount has, in some inscrutable way, to be correctly estimated by the mother bee. A slight deficiency in the amount would probably not be fatal to the life of the grub, but would rather result in dwarfing the size of the insect produced; hence the physique of the race would seem to depend in great measure upon the accuracy of the mother's estimate of the amount of food her offspring will require.

When the pea-like globule of bee food has been provided, the egg laid, and the cell closed, the mother's work, so far as concerns that particular member of her family, is done; her other toils consist simply of repetitions of these operations on behalf of the remainder of the brood. Many journeys abroad and much diligent work therefore devolve upon her before she has fully discharged the function she has to perform in the world; and as the collection of pollen and the construction of the burrow and its cells are dependent to a great extent upon weather, since bees have a great dislike to cold and windy or damp days, the completion of the important task of laying the foundation for next year's race will often, in the uncertainties of our English climate, become a rather protracted business. As so much outdoor work is involved, it is clear that the burrows will often be left by the solitary owner unprotected,

so that abundant opportunities are afforded to parasitic insects of various kinds to effect a surreptitious entrance. Disguise therefore would, in such cases, serve no good purpose, and accordingly we find that the parasites of the solitary bees do not mimic their hosts, but are often as unlike them as could well be imagined; they seem, in fact, in some cases to have gone to the other extreme of casting caution to the winds, and boldly advertising themselves in the brightest of colours. For obvious economic reasons the diameter of the burrow is usually not much more than sufficient to admit of the easy passage of the bees along it. This, no doubt, has some effect in determining what insects shall be parasitic upon any given species of bee, for it evidently fixes a maximum limit of size for the invader, none being able to gain entrance whose dimensions are too portly. Moreover, as in the majority of cases the stranger will be reared either upon the food stored up for the young bee or upon the young bee itself, we may expect that for this reason also the size of the parasite will not exceed that of its host.

Our British insect fauna would yield many illustrations of the above general principles, for there is probably no species of industrious bee that is not subject to parasitism of some sort or other. A few of the most remarkable of these may be given, and, as in our former paper, we will first consider the parasitism of bee upon bee. There is an insect commonly found burrowing into banks which is no doubt often mistaken for a humble bee. The female is a stout-bodied, densely hairy, black creature, with blunt round body, and thick reddish brushes on its hind legs; the male is altogether different, being yellowish brown, with long shaggy tufts of hairs on its feet. Notwithstanding that it has somewhat the appearance of a *Bombus*, it is not a social insect, though it is gregarious; by which is meant, that the species does not consist of large numbers of individuals of three sexes inhabiting the same nest, but of pairs of individuals of two sexes not living in the same nest, but each female constructing a separate burrow for her own young, though in close proximity to those of her neighbours. They may be distinguished from the social humble bees by the structure of the hind legs, on which no "corbicula" is found, such as a *Bombus* would have. This bee is called *Anthophora pilipes*, and it is one of the earliest to appear in spring time. It builds, or rather excavates, in steep bare banks or other exposures of earth, and will often take advantage of artificial accumulations of this kind. Thus I once saw a large colony which had formed their burrows in the walls of a thatched shed built up of turf, clods of earth, and dried mud; the bees were flying in swarms round the building, busily engaged in constructing their cells or provisioning their nests.

This insect has associated with it an extremely handsome parasite called *Melecta armata*, which is a bee of an intensely black colour, with ash-coloured hairs on the head and thorax, and beautiful snow-white tufts adorning in elegant contrast the sides of its otherwise jet black abdomen. It has a pointed body which is nearly bare, save for the snowy patches, and, of course, its legs do not carry brushes for the conveyance of pollen; in fact, unlike most bees, parasitic or otherwise, it appears to visit flowers very seldom. From this description it will be seen that the parasite is quite unlike its host, both in shape and adornment. There is great difficulty in accurately determining the details of the life-history of many of these bees, as will be evident from the fact that their nurseries are situated several inches below ground at the ends of narrow and dark passages, and that all the incidents of their domestic history are transacted in these retreats, remote from the curious eye of man; but it is pretty certain that in these cases of the association of bee with bee, the egg of the parasite is laid upon the store of food provided by the industrious bee, and the larva of the parasite, instead of that of the host, is nourished upon this store. Thus, from the habit of laying their eggs in other bees' nests, and leaving their young to be fed with materials furnished by a foster parent, these and other bees with similar habits are often called cuckoo-bees. It is a curious fact that there is another species of *Anthophora* very closely allied to the above insect, so closely indeed, that it needs a keen scrutiny to detect the points of difference, and that this too has a *Melecta* parasitic upon it, which again is specifically distinct from the other, though equally closely allied. Hence it would appear that the association of these bees is of long standing, and that whatever causes have produced the differences between the two hosts, have in like manner affected the parasites, though not quite to the same extent.

We have a large genus of elegant bees with bare legs and bodies, which latter are very prettily variegated with bands of brilliant yellow, reddish, and black; hence they are often called wasp bees. They are, however, true bees, and are not vindictive like the wasps, for, though the females are possessed of stings, they cannot do much damage with them. They have the additional advantage of exhaling a fragrant odour. The genus is called *Nomada*, the name being given in consequence of their roving habits; for they appear to be entirely parasitic, being in fact structurally incapacitated for the collection of pollen. Very little is definitely known of their economy, but it would appear that their eggs are laid in the nests of sober-coloured industrious bees, especially those of the large genus *Andrena*, and it has been thought that they are laid before those of the rightful owner, so that, when the latter finds the store of food she has accumulated appropriated by a stranger, she deserts the cell, and forms another, leaving the intruder in undisputed possession. Sometimes two eggs are provided for a single cell, in which case one or both of the resulting bees is often smaller than usual. These parasitic bees must, in fact, often be driven to considerable straits in their endeavours to make a fair provision for their progeny, being so entirely dependent upon charity; visits to many a burrow may be necessary before a cell can be found with a sufficient store of pollen already accumulated, and after all the egg may have to be laid in an insufficiently provisioned cell, so that, in return for their shiftless habits, they have to run the risk of great variations in size, some being much dwarfed by a deficiency of food. As a compensation for these risks, they are spared an immense amount of labour, especially in the excavation of burrows and the collection of pollen. The hollowing out of a main tunnel to the depth of six inches or a foot, with short branch tunnels opening out of it, and all by the successive snipping off of fragments of earth with the jaws, and their conveyance outside, represents for a small insect a great deal of hard labour. And then the search for suitable wild flowers, growing within reasonable distance of the nest, and the collection from them of pollen to be worked up successively into pellets, one for each cell, involves a great many journeys and a considerable expenditure of time, which must often tax the energies of the industrious bees to the utmost. From all these toils the *Nomadæ* are exempt, unless indeed the closing up of the cells after they have laid their eggs devolves upon them as some have supposed; notwithstanding the risks therefore, they must, on the whole, find the practice of parasitism pay, or they would have become extinct ere this.

The *Nomadæ* are not interfered with by their hosts,

which, on the contrary, calmly acquiesce in the appropriation which renders much of their labour abortive; hence the parasites fly freely about amongst them, and pass in and out of the burrows without opposition. An interesting observation of Mr. F. Smith may be quoted in point. The host in this case was a brownish stout-bodied bee, the female of which (Fig. 3, A) is a good deal like an *Anthophora*

Fig. 3.—A. Solitary bee (*Eucera longicornis*, female). B. Its parasite (*Nomada sexfasciata*), slightly magnified.

in shape, while the male may be easily distinguished from all other British bees by its enormously long antennæ, which are nearly as long as the body, whence its name *Eucera longicornis*, both parts of which refer to this feature; when freshly out it is a handsome insect with rich brown hairs on its thorax, but it soon fades on exposure to the sun and becomes weatherworn, old specimens appearing with dirty grey thorax and ragged wing-edges. The *Nomada* parasitic upon this is one of the largest and finest of the genus; it is called *N. sexfasciata* (Fig. 3, B), in reference to the six yellow adornments of its abdomen, which consist of three bands and three pairs of spots. Mr. F. Smith says that, whilst watching a colony of *Eucera*, the males of which were sportively flying round in circles, while the females were intent upon their maternal duties, returning every now and then heavily laden from their expeditions, he saw the parasites sometimes enter the burrows. Sometimes a laden female would return to its burrow just after a *Nomada* had entered it. On discovering the presence of the intruder she did not give battle, though much the stouter and heavier insect, but would simply retreat, fly off to a little distance, and wait patiently till the parasite issued from the burrow, when she would return to deposit her load. At a later season he unearthed some of the cells, and found the young parasites in them, two in each cell. The bees of this genus, *Nomada*, have not got their parasitism so definitely settled as those of the former genus *Melecta*; some of them are attached to a single species of industrious bee, but others are less particular, and depend upon a variety of hosts. Moreover, they do not, as a genus, confine themselves to a single genus of hosts, but divide their attentions amongst several that are of similar habits.

In the leaf-cutter bees we have a very peculiar and distinct set of insects, and, as might be expected, their parasites are equally distinct. The hosts line the cells they construct for their young with fragments of the leaves of shrubs, and everyone has probably noticed the appearance the leaves of rose-bushes present in gardens when they have been mutilated by these insects for this purpose. Semi-circular pieces are seen to have been cleanly cut out from the sides of the leaf; several such cuttings being often made from the same leaf when it is large enough. The bees themselves are not so often noticed; they belong to the genus *Megachile*, and are remarkable, not only for the leaf-cutting habit, but also because they have the whole of the under surface of the abdomen densely covered with hairs, which they use as their pollen-collecting apparatus. After visits to flowers, this part of the body may be seen to be heavily laden with the precious grains. The construction and furnishing of the cells is quite a work of art. A tunnel is first excavated to a length sufficient to accommodate perhaps half a dozen or more cells; the cells themselves (Fig. 4), which are composed entirely of leaves, are fitted into this, and placed one after the other along its length. An oval leaf fragment being placed so as to cover the end and a little of the sides of this tunnel, a number of semi-circular, neatly cut fragments are next placed round the sides in regular order and laid close, one upon another, each partially overlapping its neighbour, thus making a lining to the tunnel of several layers thick; the number of layers appears to depend upon the nature of the material in which the tunnel is excavated, and its condition as regards moisture, &c.: a specimen now before me has four such layers. A mass of mixed honey and pollen is now introduced, an egg is laid, and then some *circular* pieces of leaves brought to cover up the opening, just in the same way as the housewife puts a layer of paper over the jam in the recently filled jar; the specimen above mentioned has also four of these. A second cell is then built up in a similar way at the end of this, and so on till the whole set is complete: the whole tunnel is thus lined with leaves, and there are at regular intervals cross partitions, which are the tops and bottoms of the cells, and the leaves serve to protect the food from contamination by the walls of the tunnel, as well as to prevent loss of its more liquid component by soakage. When the larva has devoured the food, it is full grown; it then forms an oval lining of silk in the cylindrical cell, taking care to shut in the excrement which has accumulated during its larval life, between the walls of this silken lining and the outer leafy hangings of the cell. Within this cocoon it becomes a pupa.

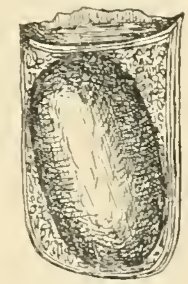


Fig. 4.—Section of cell of leaf-cutter bee (*Megachile*), with cocoon. Magnified 2 diameters.

These leaf-cutters are subject to the parasitism of a genus of cuckoo-bees called *Cœlioxys*, which are black, almost hairless, cruel-looking insects, with pointed abdomen. They present a great contrast to our last illustrations, the *Nomadæ*, for instead of the gay colours, graceful proportions, and fragrance which distinguish that genus, they exhibit a sombre and funereal appearance and a less elegant form, and give out a disagreeable odour, which, no doubt, helps to secure them from attack. It is evident that they must keep a constant watch on the doings of their hosts, in order that they may introduce the egg just in the nick of time, after the cell has been sufficiently provisioned, and before the first layer of its many-plated lid has been put on, and hence they are usually to be found in the neighbourhood of the burrows of the leaf-cutters.

(*To be continued.*)

ON THE CAUSE OF EARTHQUAKES.

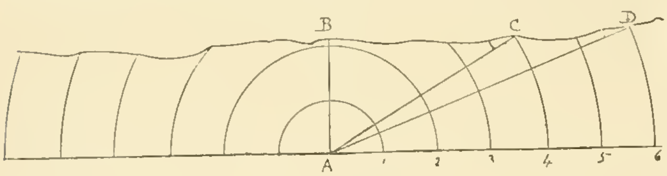
By the Rev. H. N. Hutchinson, B.A., F.G.S.

RECENT investigations have shown that *terra firma* is a phrase indicating a condition of things which, scientifically, has no existence. The crust of the earth is in a state of constant movement. Geologists have begun to study systematically the phenomena of earth-movements of all kinds, and some of the results are such as cannot fail to interest even the general reader. We all have a stake in the condition of our

planet. However, those who live in a region seldom visited by earthquakes are apt to overlook the importance of the subject. "Out of sight is out of mind," but as new methods of investigating and recording earth-tremors, or throbs, are invented, these things are brought more prominently before us. Seismology, or the study of earthquakes, has lately been making great advances, and has revealed slight movements, the existence of which was previously unsuspected.

In view of these additions to our knowledge of an important branch of natural science, we propose to say a few words on earthquakes and earth-tremors of all kinds, dividing the subject under three heads: (1) *What they are*, (2) *What they do*, and (3) *How they are caused*.

An earthquake has been defined by a high authority, the late Mr. Louis Mallet, as a wave or series of waves, of elastic compression, through the crust of the earth, in any direction, and from any given "centre of impulse." To understand this definition, think of what takes place when a stone is thrown into a pool. A disturbance is made at the place where the stone strikes the water; that spot corresponds to the "centre of impulse," the particles there communicating the movement to those next them, and these in their turn to others, and so on. In this way a series of concentric waves is produced, which get fainter and fainter, until finally they reach the edge of the pool. This is very similar to what happens when a subterranean disturbance gives a blow to the earth's crust, and a series of earthquake waves is produced from some seismic centre. But, in both cases, the waves really travel in spherical shells. (See figure.) These waves, be it re-



A, Centre of impulse; 1, 2, 3, 4, 5, 6, Waves; A C B, A D B, Angles of emergence.

membered, are due to wave-motion, like waves of sound, and are by no means waves of translation. Each particle of earth merely moves as the ears of corn move in a field when they bend to the wind, and produce waves which travel across the field before the wind. It is clear that the undulatory movements due to an earthquake shock must strike the surface of the earth at different angles, according to the distance of the seismic centre. Thus, a person who might happen at the time to be standing on the spot B in a vertical line above such a centre would feel an up and down movement, and a block of stone lying near him might be thrown straight up in the air; but if the person were some miles away from this spot, it is evident that the waves coming sideways would strike him to the ground he stands on obliquely; and "the angle of emergence" becomes less and less the further we recede from the spot lying just over such a centre. Now, it is possible, by observing the effects of earthquakes on buildings, to determine the direction in which the shock arrived, and to calculate the "angle of emergence." This is done chiefly by studying the cracks produced in buildings, and making allowance for the circumstances of each case. If, then, this angle can be ascertained for two places, and the distance between them is known, a triangle is obtained, the base of which is known, and the angles at the base; hence it is easy to calculate the depth of the centre of disturbance. Such calculations have been made in several cases, and the results arrived at are of considerable interest; for they tell us that in no case is the seismic centre at a greater depth than thirty miles. In some cases the earthquake has been found to have originated at a much less depth. If these results are trustworthy, as there is reason to think they are, the conclusion is that earthquake phenomena are not connected with the deeper-seated portions of the mass of the globe, but with those superficial portions commonly included in the earth's crust; and probably with the stratified series of rocks and their associated volcanic and plutonic rocks, rather than with the original mass which we believe to have solidified from a molten and highly-heated state.

Earthquake waves can be measured, and it is found that they are quite small, having amplitude of perhaps only a few inches. The crust of the earth vibrating in response to a seismic blow may be compared to a big bell resounding after its inner surface has been struck by the clapper. In either case the amplitude of the vibrations is capable of measurement, but the undulatory movements are not visible as we look at a sounding bell, though a marble suspended by a string and allowed to touch the bell's rim would at once demonstrate their existence by oscillating to and fro. The rebounding marble aptly illustrates the case of a block of stone being hurled up into the air by an earthquake wave. It is known that sound travels with different velocities through different substances, according to their compactness and elasticity. Hence we need not be surprised to learn that earthquake shocks have sometimes been heard twice; once through the solid rock, and so up to the ear, and again through the air which transmits the waves more slowly. Mr. Mallet made some interesting experiments on the velocity of transmission of waves due to a blow, through different substances.

In *air* the mean velocity of sound-waves is 1138 feet per second, but it varies with the atmospheric temperature and pressure—in water 4692 feet, and in a bar of iron 11,040 feet per second. The movements of the ground during an earthquake are of a complicated character. In addition to the two kinds of movement which have generally been observed—namely, the upward shock, and the long undulations, spreading in all directions, like marine waves—most authorities have added a rotating or gyratory movement. This causes a twisting of the ground, which has not only been seen but felt. Humboldt says that in Chili three great palm trees were seen to twist round one another like willow-wands, after each had swept a small space round its trunk. Pinnacles of buildings have likewise been found to be twisted. The noise accompanying an earthquake often resembles that of an explosion. Since the velocity is affected by the hardness of the rocks, it follows that strata containing any hollows partly break and check the waves, as stakes driven into a shore break the force of sea-waves in a storm. Hence we find that the early Greeks and Romans dug wells to fortify some of their cities, and prevent their complete destruction. In South America the natives have long ago adopted the same plan. Springs and natural underground passages for this reason afford considerable protection to cities which are liable to be visited by earthquakes.

Much has of late years been learned regarding seismic disturbances by taking observations which give the direction of the wave or waves, its velocity (obtained from the exact time at which it reached different places), and the "angle of emergence" as previously explained. The results are mapped out, and thus an "earthquake chart" is made, somewhat resembling the "weather charts" published daily in the *Times* newspaper. This done, it is invariably found that the greatest destruction is effected

directly over the "centre of impulse," and that the waves run roughly in circles or in ellipses from such a central spot. But the shapes as mapped out are often very irregular. This will be due to the nature of the surface of the ground and of the rocks below. It appears that in mountainous countries, like Switzerland and the Pyrenees, the great undulations are propagated in the direction of the valleys. In striking against the tilted strata at the bases of mountain masses they behave like waves of a river which dash against a bank, breaking up and changing their courses, and running along at the foot of the heights in the same direction as the stream of the valley. Earthquakes, though violent in their effects, are fortunately of very short duration. The great Calabrian earthquake lasted barely ten seconds!

We must now pass on to our second question, and consider the *effects* of earthquakes. First, they make many noises, variously described as resembling explosions of mines, distant artillery, peals of thunder, roar of cataracts, &c. Sometimes the shocks are felt before they are heard. Take the famous Lisbon earthquake of 1755. Towns in Portugal were overthrown, and places even in Morocco suffered considerable damage. The undulations extended over one-twelfth of the earth's surface! Thousands of persons were killed in Lisbon, and the sea was greatly disturbed. In England, lakes and pools oscillated to and fro like water in a basin suddenly tilted. Even Iceland was affected. The sea rose in a great wave round the coasts of Britain, and ten hours after, the sea round the West Indies was greatly disturbed! Shocks occurred for some months afterwards.

Sometimes, as at Jamaica in 1692, the sea-wave does more damage than the land-wave. At Port Royal, 2500 houses were covered with water to a depth of 33 feet. It is interesting to note the behaviour of animals before an earthquake. They seem to be able to detect slight tremors of the ground which we ourselves do not notice, and which precede earthquakes. Rats and mice leave their holes. The ground is frequently rent asunder, and sometimes permanent changes of level take place.

But it is not of the effects of seismic disturbances that we wish now to speak; they are well known, and have been repeatedly described. Their nature and origin, though less clearly understood, afford more interesting matter for a brief paper such as the present. Let us therefore pass on at once to consider our third question: how earthquakes are caused. It has been shown in Switzerland that they are more frequent at night than during the daytime; and during winter than during summer. Facts of this nature seem to indicate that the contraction of rock masses, due to a lowering of temperature, such as absence of sunlight would involve, is intimately connected with whatever causes are at work in the earth's crust to produce earthquakes. Such contraction might produce dislocations in the rocks, and these would set up vibrations. Again, by burying charges of gunpowder and guncotton, and exploding them, Prof. Milne has succeeded in producing, on a small scale, phenomena closely resembling seismic disturbances. Experiments of this nature lead to the conclusion that some sudden blow, or impact, is the most frequent cause of earthquakes. But we must be careful not to assume that only one cause exists, and that all earthquakes are due to the same cause. Evidently this is not the case. During volcanic eruptions, and also previous to an eruption, the ground trembles, and rumblings are heard, as of earthquakes. In Switzerland, avalanches of snow in slipping down a mountain side cause slight earth-tremors. The occasional falling down of great masses of rock produces similar effects.

The distribution of earthquakes helps to throw light on this difficult and, as yet, rather obscure subject. Thus they are found to be more frequent in mountainous regions than in flat, low countries. They have a connection also with volcanic regions, but rather an indirect than a direct one; for it is clear that earthquakes in general are not due to volcanoes or volcanic phenomena. Some geologists wrongly considered that all earthquakes were to be regarded as incomplete or unsuccessful attempts to establish a volcano. In other words, they are not caused by the struggles and efforts at escape made by superheated steam retained at high pressure below the surface of the earth. Steam undoubtedly is the power chiefly concerned in the production of volcanoes and of volcanic phenomena. But although earthquakes are concomitants of volcanic action, they are not to be attributed generally to the same causes. Volcanoes are associated with great mountain chains, because it is only along these lines of weakness in the earth's crust—where the strata has been contorted, crumpled, folded, and cracked, over and over again, on a stupendous scale—that the masses of heated rock below the surface, charged as they are with superheated steam at enormous pressure, can find relief and come up to the surface.

By burying telephones and microphones in the earth, it has been found that slight noises and tremors which would otherwise never be noticed—unless by animals—can be detected. Transient shiverings of the earth's crust are thus found to be very frequent. Even in Britain we have a soil subject to storms of microscopic earthquakes which, in other countries, would be the forerunners of actual earthquakes. Wherever these little earthquakes occur the earth sends forth a medley of confused sounds—crackings and snappings—probably caused by the rocks creeping toward relief from the strains which urge them to change their position. Thus we begin to realize that the world is quivering like a mass of jelly! It is hardly too much to say that this method of observation has enabled us in part to perceive the constant working of the great telluric machinery which continually builds our lands! Between these tiny movements and those which cause ordinary earthquakes there is only a difference of degree. They are essentially of the same nature. By means of delicate spirit levels, the bubbles of which move very easily, certain other movements, called "earth pulsations," have been detected by Prof. Milne. All these phenomena must be taken into account if we wish to find a satisfactory explanation. Mr. Mallet submitted for consideration the following possible causes:—(1) The sudden formation of steam by water coming in contact with highly-heated rock; (2) The escape of steam at a high pressure through fissures in the rocks and its condensation on reaching the sea; (3) Volcanic explosions; (4) Great fractures and dislocations in the earth's crust, suddenly produced by pressure or contraction, in any direction. The first three of these suggestions are not sufficient to account for earthquakes which occur outside volcanic regions; the last one seems to supply what is wanted, namely, an explanation which connects earthquake phenomena with those movements of the crust of the earth which (as shown in a previous article)* raise our continents, elevate our mountain chains, and afford means of escape for highly-heated rocky matter and associated steam from those deeply buried regions where the internal and external portions of the earth react upon each other. In mountain building and the folding of strata we may look for the main cause of earthquakes. It is titanic work, and must necessarily involve innumerable

* Knowledge for June, 1891.

snappings and much slipping of rocks past each other. Probably the slipping movements —the existence of which is abundantly proved by the numerous "faults" so familiar to geologists—took place gradually, that is, only a few inches at one time; so that a single fracture may have given rise to hundreds, or even thousands, of earthquakes. There is in mountain building a chance for many slight shocks with but a small amount of motion. In the formation of such folds as those composing Mont Blanc the tremors may have been numbered by the million. If earthquakes are associated with the raising up of mountains, who shall say that they are of no use?

PLANETARY NEBULÆ.

By Miss A. M. Clerke, *Authoress of "A Popular History of Astronomy during the Nineteenth Century," and "The System of the Stars."*

The question "What is a Nebula?" put by our editor to himself and his readers in the last number of Knowledge, must have brought home to many minds, with startling distinctness, the extreme difficulty of forming a rational conception as to the real physical status of cosmical clouds and cloudlets. They lie indeed so far beyond the range of our immediate experience, that observations of them are apt to become more perplexing the more they are rendered minute and detailed. Speculations regarding their nature have hitherto been sternly checked by the collection and verification of facts. They flourish only under the shelter of a certain amount of vague generality, while the test-questions which every theory worthy of the name must sooner or later put to experience, have so far uniformly been answered in the negative. Added knowledge has not, accordingly, in this direction, brought clearer understanding. In the registration, for instance, especially by photographic means, of lines in the nebular spectrum, and in the accurate determination of their places, much has of late been done; but their interpretation remains as backward as ever. Nebular chemistry has scarcely yet entered upon the path of progress; what it has done hitherto has been mostly to put a veto upon mistaken identifications, leaving affirmative propositions to the future. It is acquainted with but one terrestrial element; almost the only positive assertion warranted by the evidence at its command is that hydrogen is extensively present in the "fire-mist" of the skies, and present in a state of high molecular agitation. This indeed is a most important piece of information; the possibility of obtaining it proves nebular to be separated by no impassable gulf from terrestrial conditions, and gives good hope, accordingly, of eventual advance along this line of investigation.

A peculiar interest, then, attaches to the detection and demonstration of structural resemblances between nebulæ and other heavenly bodies. Mr. Ranyard pointed out some few years ago the faithful imitation of solar prominence-forms by some of the gigantic outgrowths of shining fluid from the trapezium of stars in Orion; and the resemblance is accentuated by the undoubted presence in the nebula of the solar element helium. Cometary analogies, on the other hand, are backed by no well-ascertained chemical coincidences; yet they are prevalent and remarkable. The wings and wisps, the tails, rays, and trains, the complicated varieties of tenuous veils and envelopes that develop together or in succession during the rapid sweep of a great comet round the sun, are reproduced over and over again, with endless modifications, in the "lucid matter" of remote space. But they are stereotyped in being reproduced. Forms that might well be concluded to be purely transitional, and that are in the highest degree suggestive of evanescence, are nevertheless maintained, decade after decade, without appreciable modification. Processes of change are no doubt meanwhile progressing ceaselessly, but on so vast a scale in space, and at so leisurely a rate in time, as completely to baffle human observations during the short span available for them. Hints as to their character may, however, in the course of years, be gathered by studying the distribution of matter brought about by their action. The curious discovery, moreover, has of late been made that data on this subject may differ widely, and lead to widely different conclusions, according as visual or photographic means are employed to procure them. This incongruity between effects to the eye and impressions on the sensitive plate first became apparent in M. Trépied's photograph of the annular nebula in Lyra (Knowledge, vol. xiii., p. 253); it has now been found to characterize two well-known planetaries, and may be inferred to belong, more or less, to all members of the same class.

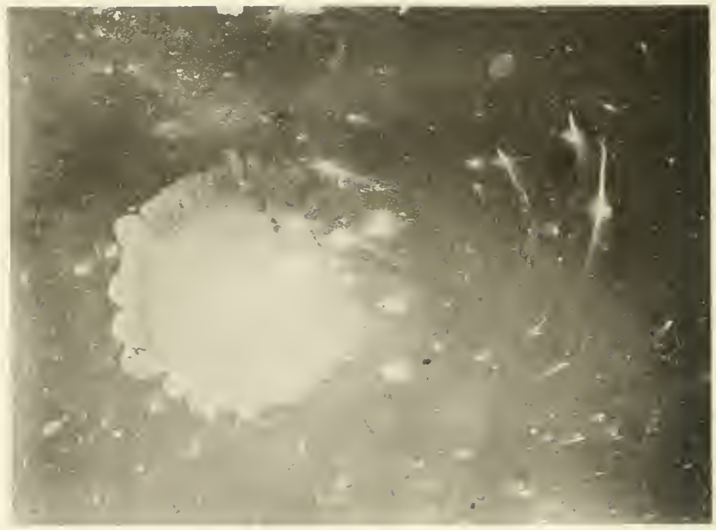
The distinction between annular and planetary nebulæ has been to a great extent abolished by the use of improved optical appliances. Each kind seems to be made up of three essential parts; a faintly shining disc—or globe projected into a disc—a ring-like condensation near its outer margin, and a central nucleus, presenting the appearance of a star. This last feature is often *seen* only with extreme difficulty, but there is reason to believe that it always exists. Mr. Burnham, who has measured a large number of these objects with the Lick 36-inch, for the purpose of providing a standard of comparison for the determination of their possible future movements, goes so far as to suggest that the presence of a central star should be regarded as the criterion of classification for planetary nebulæ.* He adds:—"Various powers have been used in studying these central stars, and particularly the brighter ones. In no instance has any one of these stars presented under any power any peculiar appearance. So far as it can be determined in this way, they all appear to be true stars, differing in no sense from the comparison stars. Many of the nights on which these measures were made were of the best quality, and any nebulous or other unusual appearance should have been apparent if it really exists."

Nevertheless, photography has a very different story to tell, as we shall see presently.

A small bluish disc observed by the elder Herschel near the star ν Aquarii was estimated by his son as equal in light to a 6·7 magnitude star, and took its place as No. 4628 in his General Catalogue. It is No. 7009 of Dreyer's New General Catalogue. With the Parsonstown reflector, in 1850, the surface of the nebula appeared tolerably uniform; it was interrupted by no certifiable perforation, and emblazoned by no central star. It gave the effect, however, in 1873, not of strict ellipticity, but of being made up of two overlapping circular segments.† It was seen, besides, to possess a pair of singular appendages like the "ansæ" of the ringed planet in our own system; and Mr. Lassell's first impression of the object at Malta was that of "a sky-blue likeness of Saturn." An interior ring, too, measuring 26″ by 16½″, and projected upon a hazy background, was plainly visible; while a small star marked the middle-point of the entire formation. Yet the great Vienna refractor failed, in 1883, to display the star to Vogel, although a power of 1500 brought out complexities of internal nebulous arrangement strongly suggestive of an essentially spiral structure. The results of Professor

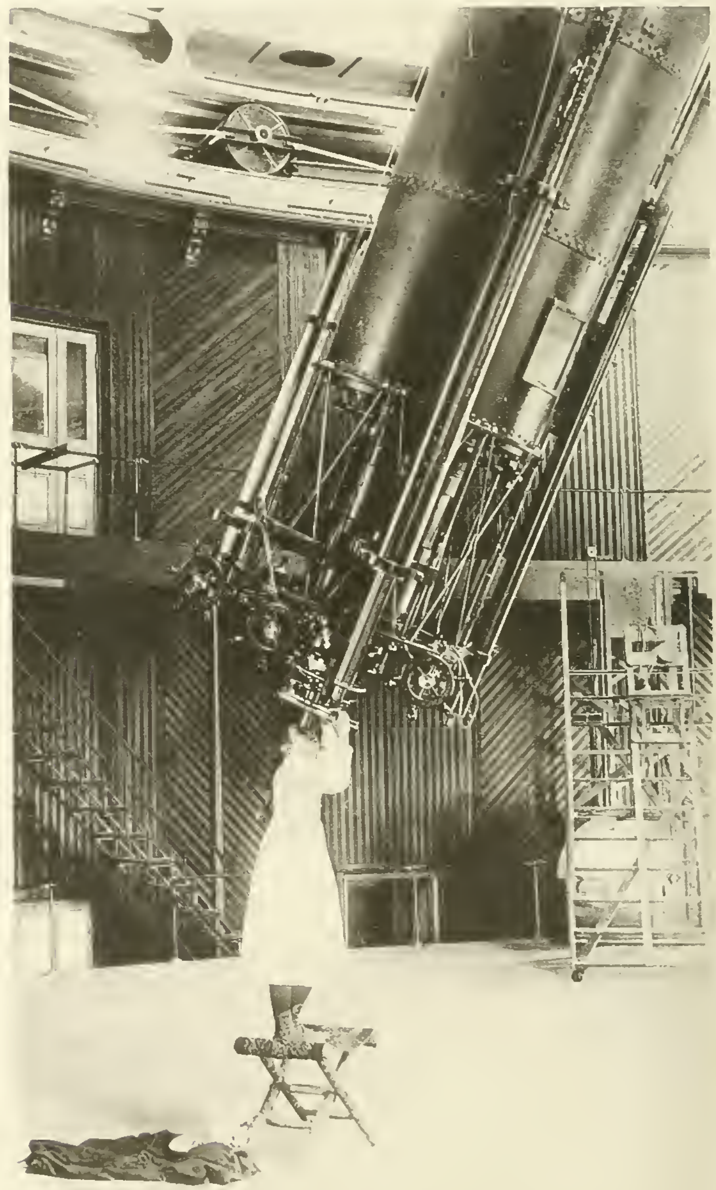
* *Monthly Notices*, vol. lii., p. 31.
† *Trans. R. Dublin Society*, vol. ii., p. 159.

Photograph of Lightning Flashes, taken by Mr. F. H. Glew on the 28th June, 1892, with a lens mounted on the hammer of an electric bell, vibrating about nine times per second.



Photograph taken during a heavy thunderstorm at Newcastle on the 17th July, 1891, at 10.15 p.m., by Mr. W. F. Dunn, and believed by him to be a photograph of Ball Lightning.

The nearest house to the Lick Observatory, from a photograph taken by Mr. S. W. Burnham.



The eye-piece end of the great Lick 36in. Refractor, from a photograph by Mr. S. W. Burnham. The camp-stool is standing on the movable floor of the Observatory, which can be raised or lowered by hydraulic machinery.

Holden's scrutiny of the "Saturn planetary" in August, 1888,* with the Lick 30-inch, deserve careful consideration. He found it to consist mainly of a star, or nebulous nucleus, surrounded by an elliptical ring lying upon an oval shield much less vividly luminous, while two exterior nebulous patches, situated nearly in the prolongation of the major axis of the ellipse, seemed to be connected with it by faint gleams of phosphorescent illumination. These represented the "ansæ" of earlier observations, and indeed their detachment from the main body had been suspected at Parsonstown in 1852, and pretty well made out in 1862. Possibly, they are embryo-satellites of the nebulous system they are still partially attached to; nor can their position relative to its longest diameter be easily regarded as accidental. We are irresistibly led, on the contrary, to trace an analogy between them and the nebulous effusions from the extremities of the major axis of the annular nebula in Lyra, and to infer in both cases the genuinely oval shape of the objects presented to our view. For why should a mere perspective effect be emphasized in any way by physical configuration?

The nebula in Aquarius was perceived by Professor Holden as of a pale blue, but its stellar nucleus as white; the difference of colour being in fact so decided as to require a change of focal adjustment in passing from one to the other. The interior arrangements of the nebula were evidently extremely intricate. The central oval, instead of being bounded by a smooth curve, "looked like an elastic link which had been warped." A sudden failure of light in the glimmering ring near the southern extremity of its minor axis enhanced the effect of distortion; but a helical form, though suggested, could not clearly be made out.

A strikingly similar object is situated in the constellation Andromeda (N.G.C. 7662). Imperfectly seen at first as a uniform, greenish-blue disc, an interior vacuity detected at Parsonstown betrayed its true nature to be rather annular than simply planetary. Nor is the ring it includes by any means symmetrically shaped. Lassell considered it to be bi-annular; Professor Vogel was impressed with the warped and twisted aspect of what may conceivably prove to be a multiple combination of rings thrown off in various planes. Closely-wound spiral branches, and a central star, were perceived with the Rosse reflector; but the object appeared starless to Dr. Struve in 1847, as well as to Searle, using the Harvard 15-inch refractor, in 1866. Lassell gave 32″ by 28″ as the dimensions of the outer ellipse; and saw the "central star" in the guise of a minute planetary disc, tinged with blue. To Vogel, however, it was entirely invisible, in spite of his best efforts for its discovery. Yet he noticed the sparkling appearance which had misled Father Secchi into the belief that he held in the field of his equatorial a "magnificent ring of stars."† For the "horse-shoe of star-dust" in Andromeda, like its *alter ego* in Aquarius, gives the usual gaseous spectrum of planetaries, which is now known to include, as a fourth visible line, the blue ray of wave-length 469, distinguishing stars of the Wolf-Rayet type.

The photographic study of these two nebulæ, lately set on foot by Dr. Scheiner at Potsdam, may be expected to add much and rapidly to our knowledge of their nature and conformation. The images obtained of them, although only half a millimetre in diameter, show a considerable amount of detail. They confirm the annular shape attributed to them on the warrant of telescopic observations, and bring out, with singular strength, the central nuclei which the best telescopes have not always availed to display. In the photographs these are, nevertheless, the brightest parts of each formation. Yet they are mere irregular condensations, with no pretentions to a stellar nature. The superiority of their actinic power repeats the phenomenon first brought into notice by photographs of the Lyra nebula, and seems to point to a general law. Dr. Scheiner thinks it can only be accounted for by supposing a predominant quantity of some peculiar gas emitting, in the main, highly refrangible light, to be collected in the central regions of planetary nebulæ;‡ yet the resulting nuclei, when they can be seen at all, shine with a white light, bear a star-like aspect, and probably give continuous spectra. The problem of their real constitution is thus far from easy to solve. But, whatever the secret of their photographic effectiveness, it is already tolerably evident that they play a part of fundamental importance in determining the structure of planetary nebulæ. They are perhaps the primary seats of the forces by which these interesting objects are moulded into characteristic shapes; and the circumstance may be regarded as a particularly fortunate one that the camera is so well adapted to display and emphasize their complex relationships with the nebulous masses organized, as it were, under their immediate control.

* *Monthly Notices*, vol. xlviii., p. 391.

† *Astr. Nach.*, No. 1018.

‡ *Astr. Nach.*, No. 3056.

LIGHTNING PHOTOGRAPHS.

By A. C. Ranyard.

Mr. Glew, whose lightning photograph appears at the top left-hand corner of our plate, has succeeded in roughly measuring the duration of a flash of lightning. The ingenious method he adopted will be found described in his letter published in our correspondence column. There is some doubt as to the direction in which the lens with which the photograph was taken was moving during the interval between the flashes. If, as seems most probable, the lens was moving at the time of the flash from the left-hand side of the plate towards the right, the first flash must have been the faintest, and it must have died away comparatively slowly to be succeeded after an interval, which probably was not greater than about the fortieth of a second, by a brighter flash, the duration of which must have been less than the two-hundredth of a second. Again, after a slightly longer interval (perhaps the thirtieth of a second), there came the brightest of the three flashes, which died away in an interval less than the two-hundredth of a second.

If instead of moving from left to right the lens with which the photograph was taken was moving from right to left during the interval between the flashes, the first flash must have been the brightest, and it must have begun with a faint glow which brightened gradually during an interval less than the two-hundredth of a second until it became intensely brilliant, and then suddenly ceased. The scale of our plate is not sufficient to show this, but the shading off of each of the photographic images of the flashes towards the right hand is distinctly recognizable in the silver prints from Mr. Glew's negative. Probably the duration of the discharge, and its mode of arriving at any particular point in its course, varies with the conducting power of the air or cloud or the terrestrial bodies from which it is derived. This is well illustrated by the old experiment of placing a little loose gunpowder over the interval between two wires, and dispersing it without explosion by the discharge from a Leyden jar sent through the wires, after which a few inches of wet string are inter-

posed in the circuit, and the discharge is found to be sufficiently prolonged to fire the gunpowder before it is blown away.

From what we know of the travelling of waves along a narrow channel, it seems probable that the maximum electric disturbance would lead the way, and that it would be followed by a decreasing wave, rather than that a small disturbance should arrive first and be followed by an increasing disturbance, but it is desirable that this question should be set at rest; possibly Mr. Glew, or somebody in the future, may succeed in taking lightning photographs with a lens moving uniformly in a circle of sufficient size, with respect to the photographic plate, to enable us to determine the part of the circular path which was being described at the time that the photograph was taken.

The photograph at the top right-hand corner of our plate is believed by Mr. Dunn, who took it, and by his father, who was standing beside him at the time, to be a photograph of ball lightning. At about a quarter past ten on the evening of the 17th of July, 1891, Mr. W. F. Dunn and his father were standing at the open window of an upper room of their house in Westmoreland Road, Newcastle-on-Tyne. A severe thunderstorm was passing over the town, and the son had his camera pointed out of the window ready to take a photograph. The window overlooks the valley of the Tyne from a point above the Elswick Works. Suddenly the father saw a ball of fire, which appeared to him to be over the river, and to be moving in an easterly direction down towards the sea—not very rapidly, but faster than a man could run. The ball appeared to Mr. Dunn to be about two feet in diameter, and when nearly opposite to the house it stopped and then disappeared. Mr. Dunn thinks that about the time it stopped he called to his son to make an exposure. The son says that he took off the cap of the camera for an instant, and replaced it. The plate was developed in the ordinary way. It was by no means the first plate which Mr. Dunn, jun., had developed. He had been practising photography for some months, and had obtained several successful photographs, and never before or since has developed a plate which showed a patch or streaks similar to those found upon this plate. Both father and son have made statutory declarations before a magistrate as to what they saw and believe with respect to the photograph, and they have kindly submitted the statutory declarations, as well as the original negative, to me for examination. I have also had an opportunity of asking Mr. Dunn, sen., some questions as to what he remembers. He says that the ball of fire appeared to be about twice the diameter of the moon. If it was two feet in diameter, this would correspond to a distance of about 115 feet; but the place over the Tyne where the ball of fire appeared to be is certainly at a much greater distance than this from his house. The camera with which the photograph was taken has a focal length of about $5\frac{1}{2}$ inches; consequently, a photograph of the full moon taken with it would only have a diameter of about $\frac{1}{20}$th of an inch, but the bright patch which he believes to correspond with the ball lightning has a diameter of $1\frac{1}{4}$ inches on the original negative. Therefore, if it represents the ball of fire, the ball must have had a diameter of about 15°, and it must have either been much nearer to the window than Mr. Dunn supposed, or its diameter must have been much greater than two feet. I am far from feeling convinced that Mr. Dunn has succeeded in photographing ball lightning, but it seemed desirable to reproduce his photograph, with such particulars as I could gather; for a photograph showing somewhat similar streaks and patches was shown to me in 1889. It had been taken during the thunderstorm which broke over the metropolis with great violence on the 6th of June, 1889, and was believed by the young man who took it—and who kindly called upon me and allowed me to question him—to be a photograph of lightning, though I was inclined to attribute the marks upon his plate to want of care in developing.

The two lower photographs shown on the plate published with this number of Knowledge are copied from photographs sent me by Mr. S. W. Burnham, who, our readers will regret to hear, is about to leave the Lick Observatory and return to his old profession in Chicago.

Letters.

[The Editor does not hold himself responsible for the opinions or statements of correspondents.]

To the Editor of Knowledge.

Dear Sir,—I beg to enclose a photograph of a discharge of lightning. The photograph was taken about 9.50 p.m. on Tuesday, June 28th, 1892. The camera was pointed S.E. from the door, at 156, Clapham Road. The lens was secured to the hammer of an electric bell, giving nine complete vibrations per second, the amplitude being about $\frac{1}{4}$ of an inch. It is evident that the main flash and its side forks all took place in less than the half of one vibration, and the motion of the lens separated the three component flashes, which to the eye would have appeared to be superposed. I compute from measurements that the whole discharge occupied about the $\frac{1}{20}$th part of a second, and the interval between the components about $\frac{1}{40}$th of a second. It will be seen that the right-hand side of each spark is fainter than the left, showing that the duration of each component was considerable, and not of the same intensity throughout its existence. The original negative shows this a little clearer than the print. Thomas's "Sandell" plates were used, which being thickly coated, in separate layers, has prevented halation defects. Some of the shadows of chimney-pots are due to sheet lightning previous to the grand flash. The vibrations of the lens were in the same plane as the plate, and in the direction of its length.

Yours faithfully,
156, Clapham Road, London, S.W., F. H. Glew.
July 11th, 1892.

P.S.—It will be seen that one portion of the flash is converted into chain lightning, this being due to the motion of the lens being almost in the direction of the length of this part of the flash, and in consequence of this the different curves have been more or less superposed in direction of length, in the form of a chain. I think this throws some light on the so-called chain lightning as seen by the eye, for if the duration of a compound spark is so much as $\frac{1}{20}$th of a second, it is possible for the eye (corresponding to the lens of the camera) to move through a certain distance, and if this movement of the eye happens to be in the direction of the length of the compound spark, then its components will be drawn out into a chain-like structure or complex ripple. Perhaps the duration may be even greater than $\frac{1}{20}$th of a second in some cases, and the eye may be set in motion by the first component of the flash. As the tendency of the eye would be to move in order to bring the object into the best position for distinct vision, so I think the effect of chain lightning might be formed in this way, or by accidental motion of the eye of the observer during the display.—F. H. G.

DOUBLE STAR ORBITS.

To the Editor of Knowledge.

Sir,—I am glad to hear that Mr. See is about to publish a fuller exposition of his views as to the orbits of double stars. I ask leave to add a few words, however, in order that my objection may be more completely dealt with in his forthcoming work. I think I am correct in saying that equality (or a near approach to equality) of mass is *one* of the circumstances which, in his opinion, tends to produce the high eccentricity of binary star orbits; and if this be so, we ought to obtain *on the average* a higher eccentricity for binaries with equal than with unequal masses. I did not, of course, intend to apply this reasoning to each individual case, but I think the average in the two cases is about the same.

A point of considerable interest raised by Mr. See is that of the age of a binary system. It would follow, I think, from his principles that the orbits of the close binaries recently discovered by the spectroscope should be pretty nearly circular. Further spectroscopic observation may throw light on this subject. All the spectroscopic binaries or variables of the Algol type have, so far as I know, spectra of the first type. From this we might, perhaps, infer that binaries with this spectrum, like Sirius and Castor, were of more recent date than binaries with spectra of the second (or solar) type, and that consequently their orbits should be on the average less eccentric. I doubt whether this is so.

We are, unfortunately, unable to observe the effects of nearly equal masses in the binaries within the solar system. The earth and moon are not far from the closest approach in the case, but the ratio of the masses is eighty to one. Is the mass of Neptune's satellite known?

Truly yours,
W. H. S. Monck.

Notice of Book.

Island Life. By Alfred Russel Wallace. Second Edition. (Macmillan & Co.)—It is about sixteen years since Mr. Wallace laid the foundations of that branch of combined biological and geographical science with which his name will always be associated, by the publication of his great and comprehensive work on the "Geographical Distribution of Animals," a work followed in the course of four years by a smaller volume containing a further exposition of his views with regard to insular faunas and floras, and entitled "Island Life." Now, after a further interval of twelve years, we have a second edition of the latter work, and it is pretty good evidence of the general soundness of the principles originally enunciated that the author has practically nothing to retract, and scarcely anything of his original arguments even to modify. The changes consist chiefly of additions to the stores of facts already recorded about the plants and animals of Great Britain, Japan, the Galapagos, the Sandwich Islands, Borneo, Madagascar, and New Zealand, and these are the results of observations made since the issue of the first edition. No better illustration could be given than this book affords of the marvellous change that has been introduced into biological studies, and the immense amount of additional interest that has been imparted to them by the abandonment of the idea of the fixity of species, and the acceptance of the principles of natural selection and evolution. To one who imagined that each species had been specially formed in the region it now inhabits, and had always existed there since its first formation, what could have seemed more dry and uninteresting than a catalogue of the fauna and flora of a district? On the other hand, to the thoughtful evolutionist who recognizes that the whole of the present condition of things is a product brought about by the inter-action through long ages of various more or less opposing influences, the very reverse is the case, and every name in such a list becomes encircled with a halo of romance as the questions arise, How came this organism to be what it is, and where it is? What past changes in its surroundings does it bear witness to? How can it be used to help to unravel the tangled skein of the primeval history, both of its race and of its present dwelling-place? And these are the questions which are ever kept in view by the author of "Island Life," whence arises in the experience of the reader a mental invigoration and stimulus as he follows the clear exposition of the argument.

Islands, in consequence of their restricted area and definite boundaries, and the barrier the surrounding seas offer to the migration of many organisms, are specially well adapted for the study of questions connected with the distribution of animal and vegetable life. But from the point of view of the naturalist, there are remarkable differences between islands, depending upon the time and method of their origin. The British Isles well illustrate one of these types, that of the recent continental, the comparative shallowness of the water that separates them from the Continent indicating that their severance took place at no very remote period. The plants and animals accordingly show a close resemblance to those of the Continent, and there does not appear to have been time enough for the production of more than a few distinct forms. From our poverty in indigenous species of the larger animals, Mr. Wallace argues that the connection of Great Britain with the Continent, after the last glacial period had exterminated the previous and richer fauna, was of short duration; thus, while Germany has nearly ninety species of land mammalia, Britain, having been early cut off, has only forty; it has also a still smaller proportion of reptiles and amphibia. In none of these cases has the effect of their isolation been such as to produce a distinct species or even variety. Amongst birds, however, we have the red grouse of the northern moors as a peculiar species, as well as two peculiar varieties of tits; while our lakes yield a goodly number of fishes which are found nowhere else. The insect fauna exhibits more striking peculiarities. A list of eighty-nine species and varieties of butterflies and moths is given, which, so far as present knowledge goes, are confined to Britain. These include the splendid "large copper" butterfly, the glory of the old collectors, but now, alas, extinct. Of beetles, again, sixty-seven are enumerated which have not been discovered elsewhere. Too much reliance, however, must not be placed on these numbers, for it may well be that many of these insects, especially the smaller species, have hitherto been overlooked on the larger area of the Continent, and may be expected yet to turn up there. This seems the more likely to be the case, inasmuch as, although fifteen additional species have been included since the first edition of "Island Life" was published, there have been a still larger number removed from the old list in consequence of their having been since found on the Continent, so that the total number of apparently peculiar beetles is actually reduced from seventy-two to sixty-seven. Whatever allowances be thus made, however, there still remains, as Mr. Wallace points out, enough to distinguish our insular fauna from that of the Continent.

The case of Madagascar is a great contrast to this; here we have an island separated by deep water from the adjacent continent, indicating a much more ancient separation, and consequently there is much greater dissimilarity in the types of both animal and vegetable life; in fact, the

fauna and flora of Madagascar, though showing African affinities, is remarkable for the extraordinary differences which exist between it and that of the continent opposite. Hence, Mr. Wallace concludes that it was severed from Africa before that continent received its present characteristic assemblage of plants and animals, which apparently migrated into it from more northerly parts. Madagascar is thus regarded as having preserved its lemurs, tanrecs, and other peculiar animals, as relics from a time dating back at least to the Miocene period, before Africa itself had received the baboons, antelopes, hippopotamus, &c., which now distinguish it, but which are absent from Madagascar.

The Sandwich Islands form a good type of the third division, that of oceanic islands, which have never been connected with any continent at all, but have been formed, usually either as volcanoes or coral islands in mid-ocean. In such cases we of course find no indigenous land mammalia, since there would be no means for their conveyance thither, and whatever forms of life the islands have received may be expected to have come from various quarters, the winds and ocean currents being the chief means of their introduction. Hence we find a somewhat mixed assemblage, showing no close conformity to those of any one particular continent. The flora of the Sandwich Islands is very rich and extremely peculiar, there being, out of 705 flowering plants, no less than 574 which are quite peculiar to the islands. Some of these, belonging to genera which in other parts of the world are low herbaceous plants, attain the dimensions and habit of shrubs, and even sometimes of trees; such is the case with certain lobelias, violets, and compositæ.

In connection with New Zealand a most interesting sketch is given of probable geological and geographical changes in the remote past, by which Mr. Wallace proposes to explain the peculiar and puzzling character of the present flora and fauna of the islands. According to this theory New Zealand received the ancestors of its recently extinct gigantic wingless birds, the moas, and of the present rapidly disappearing apteryx or kiwi, by a connection with north-eastern Australia, while that part of the island was still unconnected with its western half, and had not yet received its characteristic marsupial fauna, a route by which also it received that part of its flora which is tropical in character. Recent information as to soundings round New Zealand, and the discovery of fossil tertiary and cretaceous plants in both New Zealand and Australia, enable the argument here to be more fully elaborated than was possible in the former edition.

RUMINANTS AND THEIR DISTRIBUTION.

By R. Lydekker, B.A.Cantab.

FROM early times we find the function of ruminating, or "chewing the cud," recognized as a peculiarity of the group of mammals known in semi-popular language as ruminants. Thus in Deuteronomy the animals permitted for food are those that "chew the cud and part the hoof"; while the swine "which part the hoof but do not chew the cud" are forbidden. On the other hand, the camel, which chews the cud but has not paired hoofs, is in the forbidden list. In the permitted animals we thus have a recognition of the group of ruminants as represented by oxen, sheep, and deer; of which no better short definition can be given than that they chew the cud and have each foot furnished with a pair of hoofs symmetrical to a vertical line between them. The want of the paired hoofs in the camels, which are also cud-chewers, shows, however, that these two characteristics will not hold good for the entire group. As we proceed, we shall find that there are structural features, common to the group, in addition to the peculiarity of rumination; but, before going further, we may observe that the recognition of their paired hoofs, coupled with the absence of rumination, is an exact statement of the relationship of the swine to the true ruminants.

The word "ruminant" comes from the Latin *rumen*, which was applied both to the "cud" and to that part of the stomach in which the latter is contained previous to chewing. The Greeks had a word *meruko*, or *merukizo* (from *meruo*, to revolve), to express this action of cud-chewing, and a derivative from the former was used by Aristotle to designate ruminants, who thus first distinguished the group by a definite name. This early recognition of the ruminants as a group is probably due to their importance to man, the Biblical record showing that they yielded the only mammalian food permitted to the Hebrew, and this pre-eminence as a source of food has scarcely decreased to the present day. They are, moreover, now the dominant type of larger mammals, as witness the herds of bison which lately roamed over the American prairies, and the droves of antelopes on the African "veldt."

Commencing with the function of rumination, we may observe that it is a re-mastication of grass or other vegetable food, swallowed almost as soon as plucked, and transferred to a special receptacle in the stomach. From this, it is regurgitated into the mouth by a reversed action of the muscles of the throat, and, after having undergone mastication—or rumination—is transferred to the digesting part of the stomach. Now, it is evident that this complicated arrangement, so different from that of other animals, must be of some special advantage to the ruminants. As a matter of fact, these animals, like other large herbivora, are obliged to consume a large quantity of food to obtain sufficient nutriment; and it is obvious that if this food had to be masticated as soon as plucked, the operation of feeding would be very protracted; but by the arrangement mentioned the requisite amount of food can be gathered within a comparatively short time, and the animals can then retire to ruminate in concealment. It is superfluous to comment on the advantage this is to creatures which, like many ruminants, have but little means of defending themselves against carnivorous foes; but we may mention that many still further increase this advantage by feeding only at dawn or evening, when they are far less conspicuous than in the mid-day glare. There is, moreover, evidence that when ruminants first appeared, this rapid feeding was of more importance than at the present day, since while many of the modern larger forms, like oxen, antelopes, and deer, are provided with formidable weapons in the shape of horns or antlers with which they can keep foes at bay, in earlier times such weapons were either absent or but feebly developed.

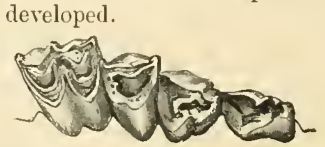


Fig. 1. The first upper molar and last two premolars of a Ruminant.

Seeing, then, that the function of rumination is correlated with a special compartment of the stomach for the temporary reception of the freshly-gathered food, it would be expected that animals thus provided would also possess an efficient masticating arrangement for reducing their food to the condition in which it yields the fullest nutriment. Such, indeed, is the case, the grinding-teeth of ruminants being of a complex structure, unknown elsewhere. In our previous article on "Teeth and their Variations," we have indicated the characteristic structure

of the grinding or cheek-teeth of the ruminants, and have shown how the last three in the upper jaw (Fig. 1) are composed of four columns, of varying height, of which the two inner ones are crescent-shaped. It was, moreover, shown at the same time how these *selenodont* (crescent-like) teeth could be traced back by gradations to the simpler *bunodont* (hillock-like) teeth of the swine. The lower grinding-teeth having their crescents directed the opposite way to those of the upper jaw, and both upper and lower teeth consisting of layers of different hardness, we can scarcely imagine a better masticating machine than is presented by the opposition of the two series of grinding-teeth of these animals. Bearing in mind this structure, the definition of cud-chewing, selenodont mammals will suffice to distinguish the ruminants from all other animals. When, however, we say that these characteristics distinguish them from all other animals, it must be added that this refers only to those of the present day. We have already seen how the Mosaic law recognized the similarity in the structure of the hoofs of the ruminants and the swine, and it is curious that while under the Cuvierian system of zoology these two groups were widely sundered, modern palæontological researches have shown that they are really closely related, the want of the power of chewing the cud, with the correlated absence of the selenodont structure of the teeth, being the chief essential features in which the latter differ from the former.

Here a curious problem is presented to those who put their faith in a mode of evolution dependent only upon so-called natural causes, in that it is impossible to give any adequate explanation of what possible advantage would be the development of an incipient selenodont structure in the teeth of the early swine-like ungulata, or at what precise stage the function of chewing the cud, with the concomitant development of a separate compartment in the stomach, was superadded to the normal mode of feeding characteristic of the swine.

Fig. 2.—Bones of the hind foot of a Ruminant. The letters indicate the lower bones of the ankle. (After Osborn.)

Here we must say a few words as to the structure of the ruminant foot. The "cloven hoof" of ruminants and swine has become such a proverbial expression that the idea may still linger that this is due to the fission of a single hoof, like that of a horse. As we have endeavoured to show in our article on "Rudimentary Structures," nothing could, however, be further from the truth; the two hoofs of a ruminant (Fig. 2) corresponding to the terminal joints of our own middle and ring fingers (or the corresponding toes), which are the third and fourth of the typical series of five. The lateral or spurious hoofs (not shown in Fig. 2) of the ruminants represent our own index (2nd) and little (5th) fingers, or toes. It is a further peculiarity of the true ruminants and camels that the two separate bones which in the swine connect the two large digits with the wrist or ankle are fused into a single cannon-bone (Fig. 2); the primary dual origin of which is indicated by the two distinct pulley-like surfaces at the lower end, which carry the bones of the digits. The peculiar little ruminants known as the chevrotains—of which more anon—retain, however, evidences of their kinship with the swine, in that some of them have the two elements of the front cannon-bone—or metacarpals as they are then called—quite separate from one another. Indeed, as indicated in the article last cited, in the same manner as we may trace a transition from the selenodont teeth of the ruminants to the bunodont ones of the swine, we may mark how the two-toed and cannon-boned ruminants passed into swine-like animals, with four toes supported by as many separate metacarpal bones.

Having now mentioned the leading characters of a modern ruminant, as distinct from other mammals, we may refer to a peculiarity, which, although by no means characteristic of all, is a striking one, and one sharply differentiating the group from all others. This is the tendency to the development of appendages on the skull, arranged in a pair at right angles to its longer axis, and taking the form either of solid branching antlers, as in the deer, or of hollow sheaths of horn covering bony cores on the skull, as in the oxen and antelopes. The distinction between antlers and horns, having been described in an earlier article, need not further engage our attention.

Passing to the consideration of the various kinds of cud-chewing mammals, we find that the true ruminants, or those with hoofs, no upper front teeth, and a cannon-bone in both limbs, arrange themselves in several minor groups. The most important to man are the "hollow-horned ruminants," such as oxen, sheep, goats, and antelopes, all of which are characterized by the presence of horns, at least in the males. The variety of form assumed by the horns render this group one of the most attractive of all animals; and we have but to recall the curved and smooth horns of the oxen, the equally massive but wrinkled ones of the wild sheep, those of the ibex with their knotted points and scimitar-like backward sweep, the spear-like form of those of the gemsbok, and the spiral twist of those of the kudu and eland, to realize the variety of contour assumed by these appendages.

The oxen (including bison and buffaloes) are, with the exception of the American bison, Old World types, and were formerly abundant in Europe, where, however, they are now only represented by the bison preserved in the forests of Lithuania and the Caucasus, and by the half wild cattle (Fig. 3) of Chillingham and some other British parks, which have been thought to be the direct descendants of the British wild ox, or aurochs, of Cæsar's time, but are more probably derived from ancient domesticated breeds which have reverted to a nearly wild state. True wild oxen now exist only in India and the adjacent regions, while wild buffalo occur both in India and Africa.

Equally characteristic of the Old World are wild sheep and goats, the "big-horn" being an outlying North American type. Both groups are essentially mountain animals, the head-quarters of the former being the highlands of Central Asia, while on the southern flanks of the same mountain-barrier the latter are more abundant. Both are also represented in the mountains of Europe; but in peninsular India there is but the wild goat of the Nilgerries, while in the whole of Africa we have only the wild sheep of Barbary and the ibex of Abyssinia. This absence of sheep and goats from Africa may, perhaps, be due to the fact that these animals are of comparatively late origin, and were probably poorly represented at the time when the other ruminants entered that continent from the

north. The musk ox of Arctic America is an aberrant form allied to the sheep.

The antelopes have a distribution nearly the reverse of that of the sheep and goats, the great majority being restricted to Africa, where there are probably fully ninety species, against about a score in all the rest of the world, except Arabia and Syria, of which the fauna is allied to that of Africa. Indeed, the only typical antelopes found beyond these regions are the black-buck, the nilgai, the four-horned antelope of India, the saiga of Tartary, the chiru of Tibet, and several members of the widely distributed gazelles. The rings marking the horns of the latter (Fig. 4) and many other antelopes are very distinctive of the group, although by no means universal. The European chamois, the goat-antelopes of India and China, and the Rocky Mountain goat of America, serve to connect the typical antelopes with the goats, and it is these alone which represent the group in Europe, to the eastward of India, and in North America. Seeing that in Tertiary times, antelopes of African types occurred in Southern Europe and India, it is difficult to determine why the group should have so dwindled or disappeared there; although we can readily account for their extraordinary development when they once obtained an entry into Africa, on account of the immense area open to them, in which there was no competition by any other ruminants except buffaloes and giraffes.

Fig. 3.—The White Cattle of Chillingham Park, Northumberland. (From Jardine.)

Fig. 4.—Horns of Gazelle. (From Günther.)

To the zoologist, Africa is indeed a country characterized by the number of animals living there which have disappeared from other regions; and there is no better instance of this survival than the giraffe, a ruminant that, as regards its cranial appendages, stands midway between the hollow-horned group and the deer. We are all familiar with the ungainly and yet beautiful form of the giraffe; but it is probably less well known that giraffes once roamed over Greece, Persia, India, and China, where, as in Africa at the present day, they were accompanied by ostriches and hippopotami. And here again we are confronted by the problem how to account for the disappearance from regions apparently exactly suited to their habits, of all these animals. The giraffe is, however, not only the sole survivor of several extinct species of its own kind,

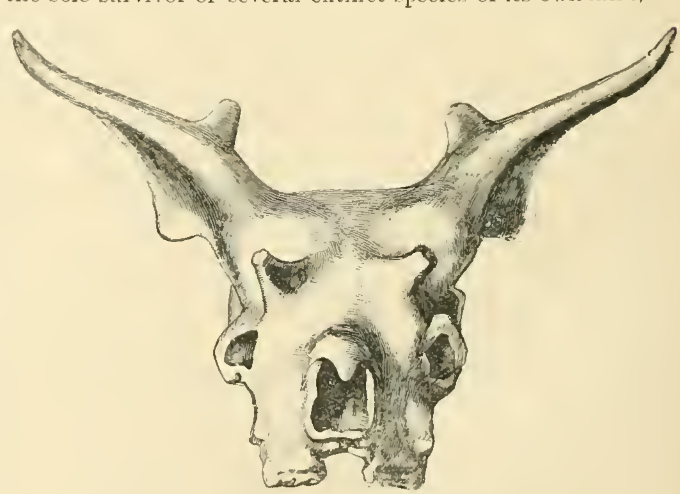


Fig. 5.—Skull of Sivathere, from the Pliocene of India.

but it likewise represents a lost group of Old World ruminants, intermediate between the horned and antlered types. The head-quarters of this group was India, where,

among other forms, occurs the gigantic sivathere, rivalling the elephant in bulk, and characterized by its two pairs of horns (Fig. 5), of which the hindmost were branching and antler-like, although apparently never shed, and were probably covered during life with skin and hair.

If our attention has been turned to Africa as the head-quarters of antelopes and giraffes, it must be directed to other regions when we come to the deer, since, with the exception of the Barbary stag, there is no representative of the group in all that continent. With few exceptions, deer are characterized by the antlers of the males, the reindeer alone having these appendages in both sexes. They are the only true ruminants found in South America, where most of the species have comparatively simple antlers, and thus show affinity with the early fossil types, some of which were antlerless. Allied species range through North America, but it is not till the north of that continent that we find in the wapiti a representative of our own red deer. The red deer group extends through Europe and a large part of Central Asia, but in India and the Malayan region it is replaced by the rusine deer, like the sambar, in which the antlers (Fig. 6, *a*) lack the bez-tine of the red deer

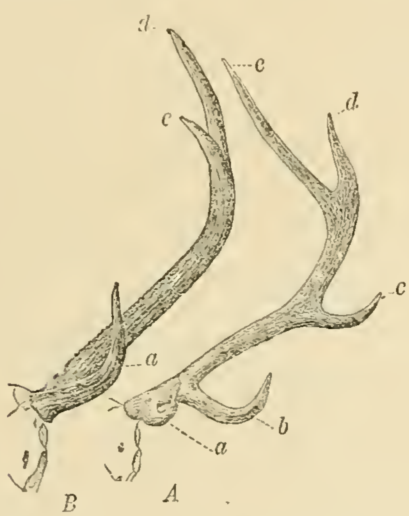


Fig. 6.—Antlers of red (A) and sambar (B) deer. *a* brow, *b* bez, *c* trez-tine, *d e* surroyals. (After Blanford).

(*ibid.*, *b*). Other marked varieties of antler are exhibited by the elk, the fallow deer, and the reindeer; but none of these approach those of the extinct Irish deer, which may have an eleven feet span from tip to tip. It is noteworthy that in a few small deer in which the males have no antlers, they are compensated by having long tusks in the upper jaw.

The tiny oriental chevrotains, and the larger African water-chevrotain, form a group quite distinct from all the above, and are in some respects related to the swine. None of them have antlers, and the African species is the only living ruminant in which the two elements of the front cannon-bone remain separate, thus affording another instance of the survival of primitive forms in Africa.

Lastly, we have the group of camels and llamas, which differ from other ruminants in that their feet form cushion-like pads, while their upper jaws possess front teeth. According to the latest researches it is considered probable that this group has diverged from primitive swine-like animals quite independently of the true ruminants, an inference which, if confirmed, is very remarkable, showing that selenodont teeth, a complex stomach, the function of rumination, and the single cannon bone, have been acquired quite independently in the two groups. The present distribution of camels and llamas is remarkable, the former being confined to Africa and Asia, and the latter to South America. Here, however, geology comes to our aid, for in former times camel-like ruminants were abundant in North America, while the fossil camels of India show certain resemblances to the llamas, and we can thus understand how the present distribution of the two sections of the group has come about. With the possible exception of some herds of the Bactrian species in Central Asia, wild camels are now unknown, and we cannot even determine the original habitat of the single-humped species.

Thus ends our brief survey of the chief groups of living ruminants and their distribution. Did space permit, we might go on to refer to their extreme importance to man, both as sources of food and of clothing, and as beasts of draught and burden, but having reached our limits, we trust that we may have aroused in our readers an interest in these highly specialized animals which may induce some of them to devote further consideration to the subject.

THE CURRENTS OF THE NORTH ATLANTIC.

By Richard Beynon, F.R.G.S.

The development of our over-sea trade, especially the Transatlantic section of it, has, during the present century, been phenomenal. Oceanic knowledge has, however, quite failed to keep pace with it. It is matter for regret that our information respecting the Atlantic, its currents, ice limits, and the meteorological conditions obtaining in the atmosphere superincumbent to its surface is far from satisfactory, and greatly behind what the importance of Transatlantic commerce leads one to expect.

We do not intend, in the present paper, to discuss the theories which best account for the formation of the great equatorial current of the Atlantic, but to deal with the oceanic circulation which lies north of its point of bifurcation off Cape San Roque. In passing, it may be stated that the western drift of the ocean in tropical regions has been known ever since Columbus made his memorable voyages across the Atlantic. That adventurous navigator states, "I regard it as proved that the waters of the sea move from east to west as do the heavens (apparently), *con los ceilos*."

The progress of the north-westerly moving section of the equatorial current of the Atlantic is so well known that it needs but the briefest allusion. It skirts the shores of Brazil, and then follows the trend of the shores of the Caribbean Sea, and so reaches the most easterly point of Yucatan. Here a division takes place, one part of its waters making the tour of the Gulf of Mexico, while the other section flows along a more direct route by the western extremity of Cuba to Florida, where a reunion takes place, and the united current, its impetus increased by north-westerly drifts, the portion of the current which has been entangled among the West Indian Archipelago, flows forth to carry vast stores of heat to the north-west of Europe.

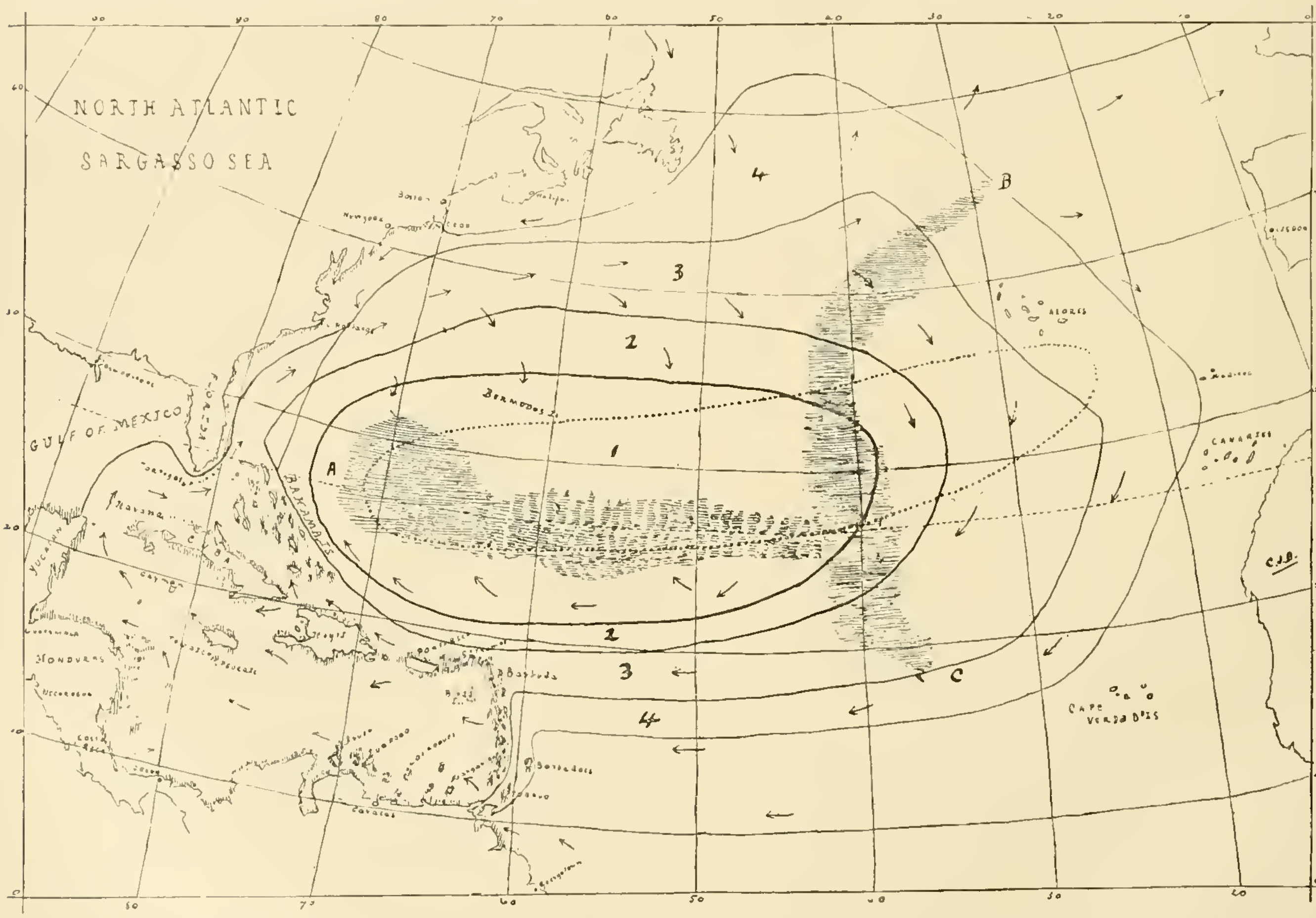
From Florida to Cape Hatteras the shores experience the full benefits of a stream of warm water, whose temperature approximates to 80° F., and whose rate of flow is some 70 or 80 miles per day. At this point, however, the eastering influence of the cold water flowing equatorwards

from the north Polar regions begins to be felt, and it is a matter of considerable difficulty to assess the value of this force, or to locate with any degree of accuracy the line along which it is applied. The general direction of the Labrador current and its ramifications was first discovered by Cabot in 1497, but although many attempts have been made to give an exact position to the line of demarcation between the waters of the Gulf Stream and those of the icy current contiguous to it, our present knowledge of the subject is yet far from being marked by scientific precision.

Navigators report that the Gulf Stream swirls by its antagonistic neighbour with a clearly defined ripple, that on leaving the former current for the latter the transition is rendered perceptible to the sight by the water changing from deep blue to light green, and to the sense of touch by a very marked reduction in the temperature. Scientific research, however, fails to establish the existence of such well-marked boundaries, although the fact of Admiral Milne, in the line of battle ship *Nile*, finding a variation of temperature of some 25° between the sea at his stern and at his bow is indisputable. The most reliable index that a vessel is in the vicinity of the meeting of the waters is the partial condensation of the aqueous vapour, for the convergence of two air columns of differing temperatures and humidities often results in dense fogs. But even this hygrometric evidence is not absolutely reliable as revealing the exact *locale* of the dividing line. The variations of the anti-trades are frequently the cause of the interposition of air columns from the Gulf Stream between the atmospheric masses superincumbent to the Labrador current. The eastern side of Newfoundland has its mean annual temperature raised considerably by its proximity to the warm current, while Nova Scotia benefits in a lesser but still marked degree. Halifax harbour is never blocked by field ice, and the formation of sheet ice in the harbour itself never obstructs the navigation of steamships. The mean temperature for the month of May averages 44° or 45° F.

Somewhere between the latitude of Cape Cod and that of Newfoundland, the Gulf current undergoes the process of division. One section flows to the north-east towards Northern Europe, while the other pursues an easterly direction, which gradually becomes south-east and then almost due south.

It will be readily seen that the difficulty of obtaining accurate information relative to the direction and speed of ocean currents is very great, and that it is only by many observations extended over a number of years that sufficient data can be procured to generalize from. The Hydrographical Department of the United States has conferred lasting benefits upon Atlantic navigators by

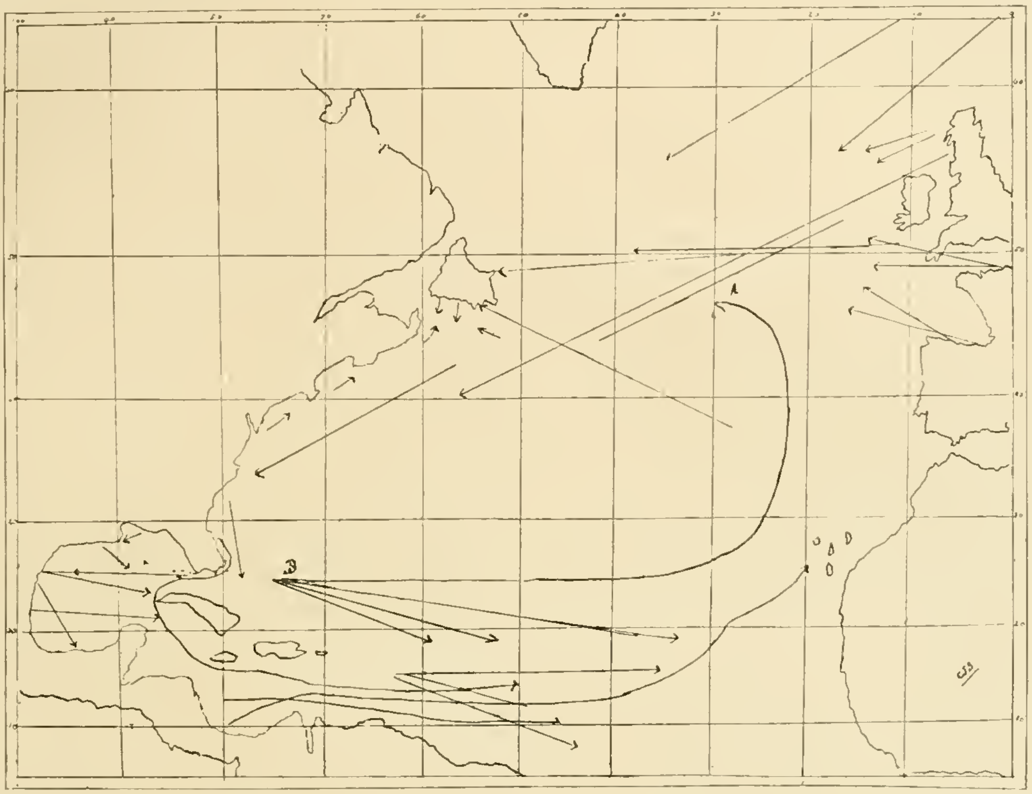


Explanation of sketch showing the North Atlantic Sargasso.—The arrows indicate the direction of the currents. The areas marked 1, 2, 3, 4 show the accumulation of different quantities of Sargasso. Fig. 1 marks the area of greatest density. The minimum of frequency is encountered in the zone numbered 4, while beyond that area weed accumulation is hardly ever noticeable. The shaded coast-line signifies the presence of a weed there identical with the weeds occurrent in the Sargasso Sea. In fact, the shaded coasts are the strips of littoral from which the Sargasso weed is detached. The dotted ellipse signifies the areas of frequent calms. The shaded portion of the North Atlantic, marked A, B, C, marks the limits of the Sargasso Sea as laid down by Humboldt.

their efforts in this direction, and not the least valuable of their contributions to North Atlantic lore is the determination, with approximate exactitude, of its currents and drifts.

The general character of the drifts has been long known. Drifted matter from the New World has been found cast upon the shores of the Old since the days of Columbus. tracks are laid down on the chart published by the Hydrographic office at Washington, and an examination of those which are laid down on the accompanying sketch-map will reveal at a glance the system of oceanic circulation in the North Atlantic.

The indraft into the Gulf of Mexico is clearly indicated, as also is the drift of the icy Arctic current, which flows



Explanation of sketch showing bottle-drifts.—The arrow-heads show the spot where the bottle was thrown overboard. Consequently, the line of drift followed will be from the arrow-head to the other unmarked end of the line. In drawing the drift-lines some attention has been paid to already-obtained knowledge relative to their direction, for it is not always correct to assume that the drifting object moves in a straight line from the commencement of its voyage to where it is picked up. This is most noticeable in the drift marked A, B, the bottle following a course almost identical with the eastern and southern boundaries of the Sargasso Sea.

The most remarkable drift on record is that effected by the ship "W. L. White." This vessel, an American schooner, was abandoned off Delaware Bay during the terrible snowstorm of 13th March, 1888. She drifted across the Atlantic, assisted doubtless by wind as well as set of current, and finally stranded, after a voyage of 5000 miles, upon one of the outer Hebrides. From March 13th, 1888, to January 23rd, 1889, was the time occupied on this drift. About 1760 A.D., an English man-of-war was wrecked near St. Domingo, and her mainmast was afterwards found stranded on the shores of the Pentland Firth.

Such instances as these, however interesting they may be, afford but little *detail* of the set of the ocean currents. These details a bottle-chart issued by the United States Marine Authorities supplies to a very material extent. The plan adopted is the throwing overboard of bottles in which is a record of the ship's name, latitude and longitude, and date. Of course the co-operation of the masters of the merchant vessels trading to the United States is necessary. The number of bottles which are traced bears but a small proportion to the numbers thrown overboard, but still they are sufficient to enable pretty accurate courses to be assigned for the ocean currents. Seventy-five bottle-equatorwards along a course lying to the westward of that followed by the Gulf Stream. The track of the Gulf Stream from Newfoundland is almost coincident with the fiftieth parallel of latitude. Looking at the eastern portion of the chart, it will be seen that the existence of a powerful indraft from the north-west into the Bay of Biscay and its enclosing shores is clearly demonstrated. It is the failure to recognize aright the influence of this current during the prevalence of south-westerly winds that has led to so many ships being cast ashore in the vicinity of Cape Finisterre. Of these disasters that of H.M.S. *Serpent* was the most terrible, but the current is a veritable danger-trap to shipmasters given to corner shaving, as the records of wreck enquiry courts show but too plainly.

The bottle-chart shows an elliptical area of ocean water whose principal axis lies along the thirtieth parallel of latitude, and this area is identical with the Sargasso Sea. From the incipience of Transatlantic voyaging down to the past few years, our knowledge of this sea has remained almost stationary. Columbus discovered it, and commented upon it, and the nautical world has since been content with his description, and has added but little to it. He reported pretty fully upon the *praderias de yerva*, or seaweed meadows, which the *Santa Maria* encountered, and subse-

quent navigators contented themselves with corroborating his statements, or striving to gain a reputation as ocean explorers, by attributing fresh wonders to the region of Sargasso. The principle underlying the accumulation of such quantities of weed as are met with, is a very simple one. To quote the words of Humboldt, "the waters of the Atlantic between the parallels of 11° and 43° are carried round in a continual whirlpool." Such being the case, drift-weed must of necessity accumulate within the area of the whirl enclosed by the circle of oceanic currents. Finality, as regards knowledge of the Sargasso or weedy sea of the North Atlantic, is yet far from attainment, but much reliable information has been obtained of late years by the researches of German scientists. The captains of the German mercantile marine have been pressed into the service as observers, and the famous *Plankton* expedition has also contributed its quota to swell the stock of reliable information relative to this Sargasso Sea. The ancient belief relative to it was that it was a "muddy, coagulated, dense sea, covered with weed which prevented the advance of a ship." Columbus nowhere affirmed that the weed materially interfered with the navigation of his vessel. Oveido, however, does, and in the hands of subsequent writers the density of the Sargasso increases until it becomes a dense mass, an effective barrier to the passage of ships. This idea is, however, long exploded, and the Sargasso Sea of to-day is simply an area of comparative calm into which drifts the weeds detached by the currents from the shallows over which they flow. This seaweed drift is densest between 70° and 40° West longitude and 20° and 35° North latitude, where the Sargasso may be said to cover the surface of the sea to an extent varying from 10 to 25 per cent. Outside this ellipse is a region where the Sargasso is less abundantly distributed, and outside that again a zone marked by the presence of still less floating weed, until to the north of the fiftieth parallel of latitude the weed ceases to be found. It is remarkable that the main axis of *the* Sargasso proper is identical with that of the region of summer calms, with the exception that the latter is continued farther to the eastward. The sources from which the Sargasso weed is derived has always been a vexed question, but the researches of Dr. O. Krummel, of Berlin, have brought the solution within a more appreciable distance than it has ever been before. The *Plankton* expedition of 1889 threw much light on the matter.

The main supplies of the *Fucus natans* of Linnæus are now pretty accurately determined. The science of sea botany has discovered that a portion of the Sargasso weed comes from the shores of the Caribbean Sea and the West Indian Islands. Other supplies are torn from the confines of the Gulf of Mexico, while some of the weed found floating in Mid-Atlantic has been identified with species attached to the shores of the north of Brazil. That the eastern shores of America produced the weed of the Sargasso Sea was long doubted. Columbus, it is said, was afraid lest the weed should mask some sunken rock, upon which his ships might strike. Others maintained that the "social weed" grew at the bed of the sea, even where the ocean was deep, and that after fructification it easily became detached from the rocks and rose to the surface. The experiments of Bouguers relative to the intensity of the light, which penetrated to a depth of 200 feet below the sea surface, were held to dispose of this theory, because the absence of sunlight in the depths of the sea was held incompatible with the existence of any but *bleached* vegetation. The species of marine weed known as *Laminaria pyrifera* has frequently been found to possess a stem over 850 feet in length.

The amount of weed discernible in any part of the North Atlantic varies with the season. Northward from 45° the scattered weed is only encountered in the late summer and autumn, while it fails completely in the spring. The further we go to the south the greater is the mean annual quantity of weed observed, and the increase is pretty equally divided between the seasons. Near 30° N. the maximum of weed is encountered in the winter, while to the southward of 25° the densest season is spring. These facts prove pretty conclusively that the Sargasso floats in summer time from the current of the Gulf Stream in a south-easterly direction. The rate which this drifting weed assumes in travelling along the convolutions of its course, until it becomes merged in the region of densest Sargasso, varies considerably according to its distance from the coast of Florida. Supposing a bunch of weeds were detached, say, from the Bahama Reef, it would require a fortnight in which to make the journey to Cape Hatteras. The next portion of its voyage—north-east, to the sixtieth degree of west longitude—would be accomplished at about half the speed, and would occupy a month to perform. From 60° W. to 40° W. the speed would have fallen to half-a-knot per hour, and the 950 knots would take ten or eleven weeks to traverse. From this point there are approximately 600 knots to be covered by the now heavy and saturated weed before the south of the Azores is reached. The weed now moves at the rate of seven or eight miles per day. When this point is reached the weed moves still more sluggishly, and gradually becomes merged in the dense Sargasso, where its only motion is an undulatory one, produced by the motions of the Atlantic swell. How long the floating weed encountered in the centre of the Sargasso Sea remains upon the surface is not known, but much of it when it takes up its position there is extremely water-logged, and soon sinks to the bed of the sea, to make room for fresh supplies of fucus.

Such is a brief summary of our present knowledge of the current system of the North Atlantic. It is far from perfect, and will probably continue to be so until the observing powers of the average British shipmaster are more scientifically developed than they are at present.

THE FACE OF THE SKY FOR AUGUST.

By Herbert Sadler, F.R.A.S.

Groups of sunspots of considerable magnitude continue to diversify the solar surface. The following are conveniently observable minima of Algol: August 9th, 11h. 26m. p.m.; August 12th, 8h. 16m. p.m.

Mercury is too near the Sun to be conveniently observed in August, being in inferior conjunction on the 26th.

Venus is a most conspicuous object in the morning sky, and is at her greatest brilliancy on the 15th, more than three times as bright as she will be at the end of the year. She rises on the 1st at 2h. 28m. a.m., or 1h. 58m. before the Sun, with a northern declination of 16° 46′, and an apparent diameter of 46″, about $\frac{15}{100}$ths of the disc being illuminated. On the 12th she rises at 1h. 50m. a.m., or 2h. 33m. before sunrise, with an apparent diameter of $38\frac{1}{2}$″, and a northern declination of 17° 11′, exactly one quarter of the disc being illuminated. On the 31st she rises at 1h. 24m. a.m., or 3h. 49m. before the Sun, with a northern declination of 17° 26′, and an apparent diameter of $29\frac{1}{4}$″, just four-tenths of the disc being illuminated. About the time of the planet's rising on the 11th an $8\frac{1}{2}$ magnitude star will be about 1′ north of Venus, and on the morning

of the 27th a 9th magnitude star will be seen very near the planet. During the month Venus pursues a direct path through Gemini, without approaching any conspicuous star very closely.

Mars is an evening star, and but for his very great southern declination would be admirably placed for observation, coming as he does into opposition (the most favourable one since 1877) with the Sun on the 4th. He is at his greatest brilliancy on the 6th, when he is distant from the earth about 35,051,000 miles. He rises on the 1st at 8h. 42m. p.m., or 47 minutes after sunset, with a southern declination of 23° 22′, and an apparent diameter of $21\frac{3}{4}''$. On the 12th he rises at 7h. 43m. p.m., or 17 minutes after sunset, with a southern declination of 24° 13′, and an apparent diameter of $24\frac{3}{4}''$. On the 31st he rises at 6h. 15m. p.m., with a southern declination of 24° 19′, and an apparent diameter of $22\frac{1}{4}''$. During the month he describes a retrograde path in Capricornus.

Jupiter is an evening star, and is getting well situated for observation. He rises on the 1st at 10h. 4m. p.m., with a northern declination of 8° 15′, and an apparent equatorial diameter of $42\frac{1}{2}''$. On the 31st he rises at 8h. 5m. p.m., with a northern declination of 8° 3′, and an apparent equatorial diameter of $46\frac{1}{3}''$. The following phenomena of the satellites occur before midnight while Jupiter is more than 8° above and the Sun 8° below the horizon. An eclipse disappearance of the third satellite at 11h. 28m. 49s. on the 1st. An occultation reappearance of the first satellite at 11h. 31m. p.m. on the 3rd. A transit ingress of the shadow of the third satellite at 10h. 39m. p.m. on the 7th. An occultation reappearance of the second satellite at 10h. 48m. p.m. on the 9th. A transit egress of the first satellite at 10h. 40m. p.m. on the 11th. A transit ingress of the third satellite at 11h. 1m. p.m. on the 12th. An eclipse disappearance of the first satellite at 11h. 47m. 57s. p.m. on the 17th. A transit ingress of the first satellite at 10h. 17m. p.m. on the 18th, and a transit egress of its shadow two minutes later. An eclipse disappearance of the second satellite at 10h. 57m. 6s. p.m. on the 23rd. A transit egress of the second satellite at 9h. 52m. p.m. on the 25th, and a transit ingress of the shadow of the first satellite at 10h. 59m. p.m. An occultation reappearance of the first satellite at 11h. 26m. p.m. on the 26th. An occultation reappearance of the third satellite at 9h. 39m. p.m. on the 30th. During the month Jupiter is almost stationary on the borders of Pisces and Aries.

Neither Saturn, Uranus, or Neptune are favourably placed for observation by the amateur.

This month is one of the most favourable ones for observing shooting stars in. The most noted shower is that of the *Perseids*, with a radiant point at the maximum display on August 10th in R.A. 11h. 52m., decl. + 56°. Observations of this region of the heavens with an opera glass will no doubt show stationary meteors, or meteors which shift their positions very slowly. Their place, and the direction of their shift, should be noted for the purpose of determining whether the radiant is a geometrical point, or a circle, or an elliptic area, as suggested with regard to the November meteors (*Monthly Notices of the R.A.S.*, vol. xlvii., pp. 69-73). The radiant point souths at 5h. 37m. a.m.

The Moon is full at 11h. 57m. a.m. on the 8th; enters her last quarter at 6h. 37m. a.m. on the 15th; is new at 10h. 59m. a.m. on the 22nd; and enters her first quarter at 1h. 29m. p.m. on the 30th. She is in perigree at 10h. 5m. a.m. on the 12th (distance from the earth 228,510 miles), and is in apogee at 1h. 3m. a.m. on the 28th (distance from the earth 251,536 miles). The greatest eastern libration takes place at 2h. 40m. p.m. on the 6th, and the greatest western at 5h. 13m. p.m. on the 20th.

Chess Column.

By C. D. Locock, B.A.Oxon.

All communications for this column should be addressed to the "Chess Editor, *Knowledge Office*," and posted before the 10th of each month.

Solution of July Problem (by J. Juchly, Munich).

Key-move, Kt × P.

If 1. . . . B to K4, 2. Q to R8, &c.

If 1. . . . Anything else, 2. Q to Qsq, &c.

There are excellent "tries" by 1. Kt to B7, B to K4! and 1. Q to Qsq, B to B5!

Correct Solution received from Alpha, who has appreciated the points of the problem.

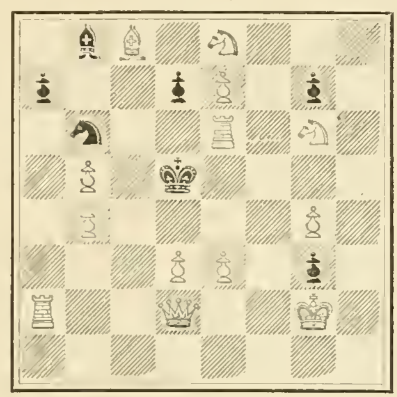
Betula.—Your problem has been carefully preserved for a solution tourney, which seems still distant.

A.B.S.—Too late to notice further. This column goes to press about the middle of the month.

PROBLEM.

By D. R.

Black.



White.

White to play, and mate in two moves.

The following game was played in the National Tournament of the British Chess Association, March, 1892.

"Ruy Lopez."

White (C. D. Locock).	Black (Jas. Mason).
1. P to K4	1. P to K4
2. Kt to KB3	2. Kt to QB3
3. B to Kt5	3. Kt to B3
4. Castles	4. B to K2
5. P to Q4 (*a*)	5. Kt takes KP
6. Q to K2	6. Kt to Q3
7. B takes Kt	7. KtP takes B
8. P takes P	8. Kt to Kt2
9. Kt to Q4	9. Castles
10. R to Qsq (*b*)	10. Q to Ksq
11. R to Ksq !	11. B to B4 (*c*)
12. Kt to Kt3	12. B to Kt3
13. Kt to B3	13. P to Q4 (*d*)
14. P takes P *en pass.*	14. Q takes Q
15. R takes Q	15. P takes P
16. B to B4	16. B to K3
17. Kt to R4	17. KR to Ksq (*e*)
18. Kt takes B	18. P takes Kt
19. Kt to Q4	19. B to Q2
20. R takes Rch	20. R takes R

21. B to K3 (*f*)	21. P to QB4
22. Kt to K2	22. P to Q4
23. P to QB3	23. B to B3
24. R to Qsq	24. R to Qsq (*g*)
25. Kt to B4	25. P to B3
26. P to QKt4	26. K to B2
27. P takes P	27. P takes P
28. P to B3	28. P to Kt4 (*h*)
29. Kt to Q3	29. P to Q5
30. P takes P	30. P takes P
31. B takes KtP (*i*)	31. R to KKtsq (*j*)
32. R to QBsq !	32. B takes P
33. R to B7ch (*k*)	33. K to K3
34. P takes B	34. R takes Bch
35. K to B2	35. Kt to Q3
36. R takes P	36. R to QR4
37. P to QR4	37. Kt to B4
38. R to QKt7 (*l*)	38. R to Rsq
39. Kt to B4ch	39. K to K4
40. Kt to Q3ch	40. K to K3 (*m*)
Drawn Game (*n*)	

NOTES.

(*a*) 5. Kt to B3, compelling P to Q3, is also good. White wished, however, to bring about a familiar variation in order to test the position arising from his 11th move.

(*b*) Probably as good as anything at this stage, provided that it is followed up as in the text. 10. QKt to B3 followed, if Black replies 10. . . . Kt to B4, by 11. R to Ksq is the usual line of play, from which White certainly derives no advantage. Black's reply is forced.

(*c*) The correct reply, as Mr. Steinitz points out. If instead 11. . . . Kt to B4; 12. Kt to B5, Kt to K3; 13. Q to Kt4, P to KB3 ?; 14. B to R6 ! followed by 15. B×P ! wins (Bauer *v.* Porges).

(*d*) Safer than 13. . . . P to B3; 14. Q to B4ch, K to Rsq; 15. B to B4: or 14. . . . Q to B2, 15. P to K6 ! with a fine game. White takes in passing in order to get rid of the majority of Black Pawns on the Queen's wing. 14. Kt to R4 was the alternative.

(*e*) White's last move was a necessary preliminary to the contemplated R to Qsq. Black cannot well preserve the Bishop, for if he retires to B2 then 18. Kt to B5 ! ; or if to Qsq, then 18. R to Qsq, with the better game.

(*f*) To prevent R to K7 after the Knight is driven away. The game is now even, but Black plays to win by advancing the Pawns. As the Pawns have already lost the option of moving either one or two squares, he can hardly do better. White waits their approach with the idea of breaking them up when within reach, reserving for his own Pawns the option of moving two squares.

(*g*) 24. . . . R to Rsq has also something to be said for it.

(*h*) This can hardly be good, but all his pieces are engaged in defence.

(*i*) Tempting though this is, it was not to be ventured without due consideration. The variations in the next note had to be worked out.

(*j*) If, firstly, 31. . . . B to R5, 32. R to QBsq ! (not 32. R to Ktsq, Kt to Q3, threatening B to B7) 32. . . . P×B (or B to Kt4) 33. R to B7ch, recovering the piece. Secondly, and perhaps best of all, 31. . . . B×P !, 32. P×B (the Rook dare not leave the Queen's file) 32. . . . P×B with an even game nearly.

(*k*) Better than taking the Bishop at once, as it secures the gain of the RP.

(*l*) A strong move, threatening R to Kt5, when Black must exchange Rooks or lose the Knight.

(*m*) Interesting possibilities result from 40. . . . K to Q4, 41. R to Kt5ch, K to B5; 42. Kt to Kt2ch, K to B6, etc.

(*n*) Against a player less a master of end-game play than Mr. Mason, White might have tried for a win by 41. R to Kt6ch, K to K2; 42. R to Kt4, etc. The attempt would have been both difficult and dangerous.

CHESS INTELLIGENCE.

The Dresden International Tournament began on Monday, July 18th. The entries were eighteen in number, and included Messrs. Blackburne, Loman, and Mason, of London, Dr. Tarrasch, of Nuremberg, M. Winawer, of Warsaw (who is probably rather short of practice), and Herr Walbrodt, of Berlin. Of the last-named player a great deal is expected on the strength of his success in a match with Von Bardeleben, who is also a competitor. The score will be given next month. Gunsberg, Bird, Lasker, and Tschigorin, to mention only European players, are notable absentees, whose place is hardly filled by the host of Austrian and German stars of lesser magnitudes.

An interesting match between Dr. Smith and Mr. T. Block has been in progress lately at the City of London Chess Club. The match is five up, and the score at one time was four to one in Dr. Smith's favour. Mr. Block, however, has since drawn up level, the score at the time of writing being four games to each player.

The City of London Club sent a strong team to Brighton early last month to encounter the Sussex Chess Association, who were defeated by $11\frac{1}{2}$ games to $7\frac{1}{2}$, one game being left unfinished, and, in the absence of the umpire, undecided.

Brighton is also the scene this year of the annual meeting of the Counties' Chess Association, beginning as usual on the first Monday in August, and concluding on the following Saturday. The programme has been published elsewhere, and it need only be said that the various tournaments for players of various strength are much the same as usual. Play takes place almost all day at the Brighton Pavilion.

Mr. Lasker has made arrangements for an American tour in the autumn. Besides giving exhibitions of simultaneous play, etc., he is willing to contest short matches with the leading Transatlantic players.

CONTENTS OF No. 81.

TERMS OF SUBSCRIPTION.

"KNOWLEDGE" as a Monthly Magazine cannot be registered as a Newspaper for transmission abroad. The Terms of Subscription per annum are therefore as follows:—To any address in the United Kingdom, the Continent, Canada, United States, Egypt, India, and other places in the Postal Union, the Subscription is 6 shillings, including postage; or 1 dollar 50 cents; or 6 marks; or 7 francs 50 cents.

For all places outside the Postal Union, 5 shillings in addition to the postage.

Communications for the Editor and Books for Review should be addressed, Editor, "KNOWLEDGE" Office, 326, High Holborn, W.C.

KNOWLEDGE
AN ILLUSTRATED
MAGAZINE OF SCIENCE
SIMPLY WORDED—EXACTLY DESCRIBED

LONDON: SEPTEMBER 1, 1892.

CONTENTS.

HOW OLD IS THE WORLD?

By the Rev. H. N. Hutchinson, B.A., F.G.S.

GEOLOGY is not one of the exact sciences with a mathematical basis, like chemistry or electricity. Nevertheless, problems arise now and then which are capable of mathematical investigation. The problem of the earth's antiquity, or rather, that of the duration of *geological time*, which is not the same thing, is one that has attracted much attention, and has led to a long controversy between certain physicists on the one hand, and geologists on the other. According to the "nebular hypothesis" now generally accepted, our planet had cooled down from a molten and somewhat viscous state long before geological time began—that is, before a watery ocean settled down by condensation from a heated atmosphere, and left our air as it now is, mostly composed of the incombustible element nitrogen, with a little oxygen, a variable amount of aqueous vapour, and a trace of carbonic acid. How many æons passed away before this state of things was arrived at, no one can say. Such times were pre-geological. But at last an ocean formed, then, perhaps later on, dry land appeared; the wind blew and the rains fell, as they do now, and the earth reached a phase which geologists believe to have been, generally speaking, not very unlike that of the present day. The question of geological time is the question of the duration of this phase. The great series of stratified rocks (including lava flows and intrusive igneous rocks, such as "dykes") were formed during geological time; and these are the pages on which the earth has recorded her history. Naturally, therefore, the geologist endeavours to seek for some means of calculating the length of time required by Mother Earth to write her autobiography.

Now the earlier modern geologists, Hutton and his followers, who, by teaching the great principle of uniformity in geological actions, placed the science on a sound and reasonable basis, and gave it an enormous impetus, were, unfortunately, so greatly impressed with this idea that they could see no trace of a beginning or sign of an end. Sir Archibald Geikie, in his recent address as President of the British Association, assembled in Edinburgh, has thus eloquently described their state of mind: "When the curtain was then first raised that had veiled the history of the earth, and men, looking beyond the brief span within which they had supposed that history to have been transacted,* beheld the records of a long vista of ages, stretching far away into a dim, illimitable past, the prospect vividly impressed their imagination. Thus the idea arose and gained universal acceptance, that, just as no boundary could be set to the astronomer in his free range through space, so the whole of bygone eternity lay open to the requirements of the geologist. . . . This doctrine was naturally espoused with warmth by the extreme uniformitarian school, which required an unlimited duration of time for the accomplishment of such slow and quiet cycles of change as they conceived to be alone recognizable in the records of the earth's past history."

This extreme teaching, in itself a reaction against the old-fashioned previous teaching, produced another reaction, and the pendulum of opinion swung back to some extent; only slightly, but still sufficiently to raise a controversy. The physicists, led by Lord Kelvin (Sir William Thomson), began to look about for some means of checking these enormous demands. Lord Kelvin considered the question of the world's antiquity from the physical standpoint. His arguments, or rather calculations, were based on three important considerations. These we must notice; but as our object in this paper is to consider purely *geological* measures of time, and his methods can only be judged by the mathematician and astronomer, we must content ourselves with a very brief account of his conclusions. Lord Kelvin arrived at a very different conclusion, and this was derived from three distinct lines of reasoning, or rather calculation. *First*, he considered the internal heat, and rate of cooling of the earth; *secondly*, the tidal retardation of the earth's rotation; and *thirdly*, the origin and age of the sun's heat.

With regard to the earth's heat: the rate of increase of temperature downward from the surface is known, for a certain distance, by observations in mines. As many of our readers are already aware, it is about 1° F. for every 50 or 60 feet. But this rate is not maintained, and becomes less at great depths. Then with regard to the earth's present temperature—about 36° F. at the bottom of the oceans. From such available data he calculated that the earth could not have consolidated, from its former molten state, *less* than 20 millions of years ago, nor *more* than 400 millions. In the one case the underground heat would have been greater than it actually is; in the other there would have been no sensible increase in temperature downwards. He afterwards inclined towards the lower limit rather than the higher one, and said that we ought to be quite satisfied with 100 millions of years for the duration of geological time. Professor Tait would even limit the

* Viz., the 4000 years of Archbishop Ussher's Chronology, a mere "pious opinion" nowhere expressed in Scripture.

period since the earth's consolidation to 10 or 15 millions of years.*

We pass on to the argument from the tides. It is generally admitted that the daily tidal waves must, in some degree, diminish the rate of rotation of the earth on its axis. Its action has been compared to that of a brake on a wheel. At one time, then, the rotation was more rapid; in other words, the earth's day was shorter, and has since been steadily getting longer. If we assume any antiquity for the globe greater than 100 million years, he thinks the flattening at the poles would be greater, owing to greater centrifugal force having been formerly exerted by the more rapid rotation.

Lastly, Lord Kelvin has attempted calculations based upon the radiation of heat from the sun, and also upon the amount of heat generated by the falling together of meteoric masses, such as by clashing together may have given rise to the sun. He admits, however, that his conclusions from this source are, from the nature of the case, less reliable. Still, like the other calculations, they point to a comparatively small number of millions of years, perhaps about twenty. The sun may, however, have continued to receive showers of meteorites, and thus to be replenished with heat; which would disturb these calculations. Moreover, certain chemical changes may be the means of liberating heat in the sun.

But we will not dwell on these difficulties here. It is hardly necessary to say that most geologists consider that conclusions such as these are too sweeping. Seeing what vast changes have taken place on the earth—so many thousands of feet of solid rock formed by slow deposition in water, so many new forms of life introduced at certain epochs, while others were extinguished—the geologist cannot bring himself to believe that all the changes (only fully realized by those who study the record of the rocks) could have taken place within 20 or even 100 millions of years. Some, doubtless, would demand much more time, and refuse to accept even the limit of 400 millions. No one distrusts the actual calculations; but many do seriously distrust the data (or want of data) upon which they are founded. Hence a serious difference has arisen between geologists and physicists with regard to the duration of geological time. Mathematics are an excellent mill, and will grind out beautiful results; but what you get out of this mill depends very much on what you put into it, and if you put in material based upon uncertain assumptions, you must not be surprised at getting a result tainted with similar uncertainty.

Let us quit this somewhat unsatisfactory region of speculation, and see what further light can be gained from the science of geology. It will be interesting to compare any results that may be obtained with those above mentioned, and to see whether they harmonize.

The geologist knows only two time-keeping processes; one is *rock formation* (deposition), the other *rock destruction* (denudation). A third is sometimes referred to, namely, changes in the organic world, involving the appearance, from time to time, of new species, genera, families and orders of plants and animals—changes which are comprehended under the one word "evolution." But this kind of change, which has been going on ever since the oldest (Archæan) rocks were first formed, concerns the biologist more than the geologist. The biologist, as Professor Huxley said, has no clock, and must take his time from the geological clock. In other words, when, on passing from one rock formation to another, a great change in the fossils is noticed—as, for instance, in passing from Primary rocks to Secondary or from Secondary to Tertiary, the lapse of time required to bring about such evolutionary change can only be gauged by the thicknesses of the strata in which the different fossils are found, and partly, in the two cases above quoted, by the "stratigraphical break" between the two sets of strata; that is, the amount of rock denuded during the interval between the two eras. As the Greeks used to detect "the lazy foot of time" by the slow dropping of water from a clepsydra, so the geologist measures his periods by the work of water, either as a rock destroyer or as a rock former. This is our water-clock, and our two measures of time are (1) depth of rock denuded, (2) depth of rock deposited. Now the condition of the water-clock's accuracy as a time-keeper was uniformity of action, that the drops should continue falling at the same rate; so with the geological clock. These two processes, so closely related to each other, must be supposed to have been working throughout geological time (that is, the time during which the great series of stratified rocks were being formed) with considerable uniformity. This brings us back to the "theory of uniformity" originated by the illustrious Hutton, and expanded and explained by Playfair and Lyell.

Readers of Knowledge will hardly need to be told that "denudation" is chiefly effected by "rain and rivers." The consequence of denudation in one place is rock formation in another; the one is complementary to the other. In other words, the *débris* of continents is carried by rivers into lakes, seas and estuaries, there to settle down and "sow the dust of continents to be." Now rivers depend for their supplies on *rainfall*; hence, rainfall is one of the main factors in problems about denudation. Geologists believe (from a mass of evidence in the stratified rocks which it would take too long to expound here) that the rainfall has, in past periods, been pretty much what it is *now* in various parts of the world—not necessarily in *Europe*. It *may*, however, have been somewhat greater as far back as the Archæan and Palæozoic times, when, perhaps, the earth was sensibly warmer and the sun sensibly hotter. Hence, geologists consider that they are justified in attempting to form some kind of estimate of former periods of time from the two processes above referred to. Not only is it possible thus to compare one period with another and to say which was the longest, but we venture to think that it is justifiable to attempt to calculate the limits of geological time on the basis of the rate at which strata may be formed. We want to translate feet of rock formed into years. To give a mathematical basis to geology is one of the great problems of the future. What degree of success awaits such efforts we cannot say, but certain attempts have been made to gauge denudation, and to see at what rate it goes on. With regard to deposition of strata, very little, if anything, has been done, and we cannot help thinking that important results might be obtained in this direction; but of that we shall speak presently.

Let us briefly consider the first operation, namely, the wearing away of land. The subject of atmospheric denudation has been arithmetically investigated, in order to ascertain at what rate a given continent, or portion of a continent,

* There is some uncertainty as to the amount of heat radiated by the sun. Assuming Helmholtz's theory as to the source of the sun's heat, that is that it is due to the slow contraction of the sun's bulk, and assuming that the sun has derived its heat solely in this way, we may say with some certainty that the contraction of the sun to its present size from a diameter as large as the earth's orbit would not have furnished more than 20,000,000 times as much heat as the sun now supplies in a year. But from theoretical considerations it seems probable that a gaseous mass losing heat by radiation, and contracting under its own gravity, must rise in temperature. So that it is possible that the annual loss of heat in former times may have been less than at present, even though the radiating surface was then greater.—A. C. Ranyard.

is at present being worn down by "rain and rivers." Take the great area drained by the Mississippi, which is what geographers call its "basin." The area of this basin is reckoned to be 1,147,000 square miles. It is clear that all the mud, sand, &c., brought down by this great river to the Gulf of Mexico must be derived from the rocks and soil in that area; the next step is to find out how much solid matter is brought down every year. Most extensive and accurate determinations upon this subject have been made by the United States Government. As the mean of many observations carried on continuously at different parts of the river for months together, Messrs. Humphreys and Abbot, the engineers employed to investigate the physics and hydraulics of the Mississippi, found that the average proportion of sediment contained in the water is $\frac{1}{1500}$th by weight, or $\frac{1}{2900}$th by volume. But, besides the matter held in suspension, they observed that a large amount of coarse detritus is constantly being pushed along the bottom of the river. They estimated that this moving stratum carries every year into the Gulf of Mexico about 750,000,000 cubic feet of sand, earth, and gravel. Their observations led them to conclude that the annual discharge of water by the Mississippi is 19,500,000,000,000 cubic feet, and consequently that the weight of mud annually carried into the sea by this river must reach the sum of 812,500,000,000 pounds. Then, taking the total annual contributions of solid matter, whether in suspension or moving along the bottom, they found them to equal a prism 268 feet high, with a base of one square mile.

But, besides all this, there is in every river a large amount of matter chemically dissolved. This consists chiefly of carbonate of lime, dissolved by rain-water in filtering through rocks before it reaches the river. Properly to estimate the loss sustained by the surface of a river basin, we ought to know the amount of mineral matter thus removed, as well as that referred to above; and to make sure of good results, we ought to have the total volume of water discharged, from measurements made at different seasons *and* extending over a series of years. Such data have not been fully collected from any river, though some of them have been ascertained with approximate accuracy, as in the cases of the survey of the Mississippi and the Danube. As a rule, more attention has been paid to the amount of mechanically suspended matter than to the amount in solution. We must therefore confine ourselves to the former, but it must be borne in mind that the following estimates are under-statements of the truth, because the amount of dissolved matter is left out. Some of the results obtained are as follows:—The Mississippi, with a basin of 1,147,000 square miles, discharges annually 7,459,267,200 cubic feet of solid matter; the Rhone, with a basin of 25,000 square miles, discharges 600,381,800 cubic feet of solid matter; the Danube, with a basin of 234,000 square miles, discharges 1,253,738,600 cubic feet; the Po, with a basin of 30,000 square miles, discharges 1,510,137,000 cubic feet. Now as all this solid matter comes off the surface of so much land, the area of which is known, it can easily be calculated what thickness of rock must have been removed (on an average) to produce the amount brought down to the sea, as given in cubic feet. On elevated land, where mountain streams run faster, more rock is removed than over low plains or gentle slopes, where rivers run slowly. But we only want a general average for the whole area. An illustration may serve to make this clear. Given a lump of butter, containing so many cubic inches, and a slice of bread, with area so many square inches; any schoolboy could find what the thickness of the butter would be when spread evenly over the bread. The results for the great rivers were as follows:—The Mississippi removes $\frac{1}{6000}$ foot from its area in one year, or one foot in 6000 years; the Rhone removes $\frac{1}{1528}$, or one foot in 1528 years; the Danube removes $\frac{1}{6846}$, or one foot in 6846 years; the Po removes $\frac{1}{729}$, or one foot in 729 years. Now these are very important results, and since the physics of the Mississippi have been more carefully studied than those of perhaps any other river, and as that river drains so extensive a region, embracing so many varieties of climate, rock and soil, we shall probably get the best results by taking the Mississippi rate of denudation as a fair one. Let us see, then, what that rate means. It means that the surface of its basin will be lowered 10 feet (generally) in 60,000 years; supposing the rate to continue, 100 feet in 600,000 years, and 1000 feet in 6,000,000 years. Apply this to the whole of North America, the mean height of which, according to Humboldt, is 748 feet above sea-level, and we find that this continent would be worn away in about $4\frac{1}{2}$ millions of years. The same kind of calculation, based upon the rate of denudation by the Upper Ganges, has been applied to the continent of Asia, and a shorter length of time was found to be required to wear it all down to sea-level. But the Ganges rate seems to be hardly a fair one; so we will keep to the Mississippi. Such calculations are made on the assumption that no serious changes take place in the way of earth-movements, raising or depressing a continent. Upheaval would undoubtedly quicken the rate of denudation, by giving greater velocity to the rivers (on account of increased fall), and in the same way depression would check the rate of denudation. But in spite of this possible element of disturbance, the result above given is an important one. Now the amount of denudation that might thus take place over the North American continent is *a mere trifle* compared to the vast denudation which *must have taken place* in order to provide the prodigious amount of solid matter contained in the whole series of stratified rocks. Their total estimated thickness is about 100,000 feet! It is clear, then, that a much greater number of millions of years was required to lay down this great series of sedimentary rocks on ocean beds, especially when we reflect that such material had to be distributed by ocean currents over vast areas, and also that many of these rocks were built up very slowly in the deeper parts of oceans by the slow accumulation of organic remains. This applies, for example, to the carboniferous limestone, the oolites, and the chalk formation.

Evidences of great denudation abound both in Great Britain and in Europe, and in all parts of the world. Thousands and thousands of feet of solid rock have been removed, and yet such phenomena were by no means spread over the whole of geological time. We can often prove that even in the interval between two successive periods enormous denudation took place, and the mind is bewildered in endeavouring to combine with any reasonable amount of time required for such intervals the much greater periods required for the accumulation of the subsequent or overlying strata. Any student who is familiar with geological sections can call to mind numerous examples of great denudation. For instance, what a vast period of time is indicated by the upheaval and subsequent denudation of the pre-Cambrian (or Archæan) rocks before those of the Palæozoic era were deposited on their upturned edges! No attempt has been made to estimate in years this interval. Or, to take another case, it is found that in many parts of our country a great thickness of the carboniferous rocks, especially the coal measures, was denuded away *before* the advent of the Secondary or Mesozoic era. Sir Andrew Ramsey has calculated (from sections drawn to scale) that a covering of rock to the depth of one mile

was removed from the surface of the Mendip Hills, and most of this destruction took place during the above interval. No one has yet attempted to apply a rate of denudation to this case, for the uncertain elements in such a problem are many. The Mississippi rate of one foot in 6000 years would hardly be applicable, being an average for a large area including mountains, valleys, and plains; whereas the Mendip Hills are a small hilly area. If we could find the rate at which some of our present mountainous regions are being worn down, and obtain an average therefrom, it might be justifiable to apply such an average to the case in point. But mountains are composed of hard and often crystalline rocks, and this fact would tend to counteract the more rapid erosion due to the velocity of mountain streams.

We will now endeavour to point out a method that might possibly lead to valuable results if followed, and from which an average rate of rock formation might be obtained.

Take the case of the Mississippi. What becomes of all the solid matter brought down by this river? It mostly finds its way into the Atlantic, for the Gulf of Mexico is swept by that powerful current the Gulf Stream. It would not be spread *all over* the Atlantic bed, for some may be carried up to the North Sea and Arctic Ocean; and again, there are large areas in the Atlantic where no *sedimentary* deposits are forming, but only globigerina ooze, pteropod ooze, or the red clay (believed to be volcanic and even cosmic dust). These areas are far from land, and some of them are the deepest recesses of the Atlantic. Suppose that all the *débris* from the North American continent were being washed into the Atlantic only. Now this ocean is larger than North America in actual area, but we may subtract the areas devoted to globigerina ooze or red clay. What these areas are could doubtless be estimated by Mr. Murray, of the *Challenger* expedition. We do not know how much they are; but let us suppose that when this is done, an area remains equal to the continent of North America. Then it would follow that all the rock material removed from that surface of land settles down to form new rocks *on an area equal to that of the land from which it came.* Now if, taking the Mississippi rate, one foot is removed from the former area in 6000 years, it follows that about one foot is added to the latter surface in the same time. It would really be rather more, because the new material would be soft and unhardened by pressure, while the old rocks from which it came were compressed and hardened before they came up to form a land surface. But this difference may be neglected. It will thus be seen that a result of some value is obtained, namely, just what we have been seeking—an average rate of rock formation.

The question arises—"Is this rate of rock formation over a large area of sea bed, viz., one foot in 6000 years, too rapid?" We are inclined to think that it is. It might apply to strata formed in shallow waters, but it seems too high a rate for those formed in deeper waters, and certainly is inapplicable to slow-growing deposits like globigerina ooze. However, let us see what we can make of it. The whole series of stratified rocks is generally estimated at 100,000 feet—taking all the formations and adding their thicknesses together. Here would be a measure of geological time, if only we knew the average rate at which they were built up. Suppose we apply the rate just obtained and see what it leads to. If one foot is formed in 6000 years, 100 feet will be formed in 600,000 years, and 100,000 feet in 600,000,000 years. Six hundred millions of years! This is more than Lord Kelvin's extreme limit for geological time, or the time since the earth consolidated from a molten state. And yet we have taken a rate of rock formation that appears not to err on the side of rapidity; and, moreover, this calculation makes no allowance for those great "gaps" or "breaks" in the 100,000 feet of the geological record with which the student will be familiar. Again, it makes no allowance for the necessarily slow rate at which organic deposits were formed, and formations of this kind occupy no small fraction of the whole series of rocks. For example, the great mountain limestone in one district is 4000 feet thick; the chalk in the Isle of Wight is 1000 feet thick; then there are the oolites between, to say nothing of Silurian limestones below.

It is therefore not surprising that geologists are dissatisfied with the limits laid down by Lord Kelvin and others. They demand much more time than he will allow, and we think that the calculation above given justifies such a demand. His later estimate of only 100 millions of years certainly seems too small. Professor Huxley, some years ago, endeavoured as it were to make peace between the two parties in this controversy by taking the latter limit of 100 millions and applying it to the stratified rocks. If 100,000 feet of rock were formed in 100 millions of years, then the rate of rock formation would be one foot in 996 years—say, roughly, 1000 years. Now the result we obtained above was one foot in 6000 years, so that our rate is six times slower than that which follows from Lord Kelvin's computation, and we venture to think that it would be more acceptable to geologists.

One cannot help hoping that before long some attempts will be made to observe the rate of deposition in different seas. Would not observations of the amount of sediment suspended in sea-water, taking samples from various depths, be useful? But it would be better still if someone would let down vessels (like rain gauges) on to the bed of the sea in various spots, leave them there for twenty years, and then take them up and measure the amount of solid matter contained in them. They could be attached to buoys by thin wire ropes; thus the sites would be indicated and they could be pulled up. Or again, perhaps in the future an international committee of scientific men may be formed to observe and measure the amounts of *débris* brought down to the Mediterranean by all the principal rivers flowing into it! It would take a long time, but the work could be divided up, and when done we should have a fair idea of the amount of sediment settling down in that area of sea, and so could calculate the rate of rock formation that obtains there.

BEE PARASITES.—III.

By E. A. Butler.

(*Continued from page* 145.)

THE parasites from which solitary bees suffer belong chiefly to two orders, the Hymenoptera and the Coleoptera. We have already seen how the stores of food they accumulate for their young are liable to be appropriated by cuckoo bees; we have now to show what persecutions are inflicted upon them by other Hymenoptera, and by some very remarkable beetles, examples of the order Coleoptera. And first as to the former. Some of the most resplendent of all British insects are the ruby-tailed flies, golden wasps, or fire-tails (Fig. 5). They constitute the family *Chrysididæ*, a carnivorous group of small extent, and not unlike small bees in shape. If they were only of larger size, they would vie with the most gorgeous productions of the tropics in splendour, but small as they are, their brilliance

and beauty never fail to elicit exclamations of delight and admiration when seen for the first time. The general type of coloration is much the same in the various species, consisting of a sparkling metallic green or blue head and thorax, and a polished ruby-red abdomen. To see them at their best, one should take up one's post on a hot summer's day by the side of a steep, bare, clayey bank, perforated with insect burrows; a specimen will soon be seen coming flashing through the sunshine and settling on the bank, where it will either rest displaying its beauty like a sparkling gem of ruby and emerald, or else run about over the surface, quivering its antennæ, and every now and then tapping the ground with them in apparent excitement.

Fig. 5.—Ruby-tailed Fly *(Chrysis ignita)*. Parasitic on bees. Magnified 2½ diameters.

The different species of this family frequent the burrows of various Hymenoptera, and amongst others those of certain bees, and lay their eggs in the cells in order that the larvæ hatched from them may devour the grubs for which the cells were made. By the bees they seem to be regarded with hostile feelings, whence the introduction of the egg becomes a hazardous proceeding. But they have a novel method of defence, which sometimes stands them in good stead. The last few segments of the abdomen, which taper away to a point, are, in a state of rest, telescoped up and withdrawn into the body, which thus appears to be more or less bluntly rounded at the end. When these segments are thus retracted the abdomen beneath is concave, and its junction with the thorax being a small one and extremely flexible, the whole abdomen can be bent under and folded back on the under side of the thorax, the insect thus becoming, but for its wings, almost globular. In such a condition, its hard and shining convex dorsal covering is all that is exposed, and enemies cannot make much more impression upon this than a dog can do on a hedgehog similarly folded up. When, therefore, hard pressed, the *Chrysis* is apt to adopt the policy of passive resistance, to fold itself up, and drop to the ground, lying motionless till the danger is past. The brilliant colours seem as though they might be protective in function, dazzling, and therefore warning off the foe; and it is conceivable that some insects or other enemies might be deterred in this way from attacking such bright objects. But if this be the case, the insect saves itself by inspiring a fear that is almost entirely unfounded, for though the *Chrysididæ* are bold and daring adventurers, they are not able to do much damage, having but a feeble sting, or rather no real poisonous sting at all, but only a sharp ovipositor which can give a slight prick. Moreover, their brilliance does not always secure them immunity from attack, as the following incident will show. Lepelletier de Saint Fargeau records that he once saw one of these insects, named *Hedychrum regium*, enter the burrow of a solitary bee, and being apparently satisfied with the arrangements within, it came out and, turning round, began as usual to enter backwards, in order to bring its telescopic abdomen into position for depositing eggs. Just at that moment, however, the bee returned laden with provisions, when ensued a scene very different from that witnessed by Smith in the case of the cuckoo bee related last month; on seeing the intruder, the bee at once pounced upon it, when the parasite fell back on its usual defence of doubling up into a ball, becoming thus invulnerable to the bee's sting. The latter, however, was not to be defeated in this way, and proceeded to bite off its enemy's wings, dropping the body to the ground. Thus victorious in the contest, it entered its burrow and deposited its load, and then went off on another expedition, no doubt reckoning that it had nothing further to fear from its humiliated foe. But the latter was quite equal to the occasion, and, as soon as the coast was clear, unfolded itself, crawled back to the burrow, and successfully accomplished its task.

There is also a large family of exceedingly minute Hymenoptera, called the *Chalcididæ*, which are parasites, and which are equally resplendent with the ruby-tailed flies, and sometimes even more so; they are mostly of a brilliant metallic green, or golden green, and may often be found in great numbers amongst long herbage of various kinds; but they are so minute that they would not be noticed unless carefully looked for. By sweeping amongst such herbage with a net, large numbers of specimens may often be found. Several members of this large family are parasitic upon bees, and, as they are so small, one bee's cell frequently contains a great number of their larvæ; in the cells of the *Anthophora*, referred to in our last paper, two species have been found together, and it seems probable that one of them is parasitic on the bee larva, and the other upon it, a by no means unusual arrangement in the family. The larvæ that feed upon the bee-grub are exceptional in being *external* instead of internal parasites; they cling to its body and suck out its juices, till it is completely exhausted.

Few more remarkable life-histories are to be found throughout the whole range of the Insecta than those of the beetles which constitute the family *Meloidæ*, and which, in some stages of their career, are entirely dependent upon bees for their support. So strange and unexpected, indeed, are the details of their metamorphoses, that they were for some years a great puzzle to naturalists, and the combined efforts of several most careful investigators were needed to clear up the mysteries. The commonest members of this family found in Great Britain belong to the genus *Meloë*, and are generally known as oil-beetles (Fig. 6), because they have the power of exuding from their bodies, when handled, a yellow, acrid, oily liquid. They are heavy-looking insects of a dull bluish-black colour, and wingless, although the elytra or wing-covers are developed as two flexible oval pieces lying over the base of the abdomen. The abdomen of the female becomes enormously distended by the development of the eggs, which, though individually minute, are extremely numerous, as many as four thousand having been estimated to be contained in the ovaries at one time. This enormous fecundity has relation to the life of hazard the young larva lives in its earliest days, in consequence of which no more than a very small percentage of those hatched from the eggs in all probability reach maturity. The eggs are laid in a cavity in the ground, which the female *Meloë* excavates for the purpose in early spring. The larvæ hatched from these are minute but very active yellowish creatures, with six legs and two pairs of hair-like appendages at the end of the abdomen (Fig. 7). As soon as hatched, they climb up the stems of flowers, such as buttercups and dandelions, lying in wait in the flowers for that chance which may help them on to the next stage in their life. Now, as the perfect insects are

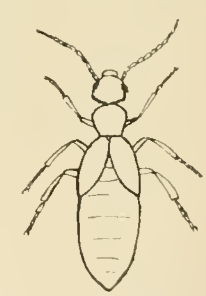


Fig. 6.—Oil-Beetle *(Meloë proscarabæus)*.

vegetarians, feeding upon flowers, especially the two just mentioned, it might be imagined that the young larvæ possessed similar tastes, and had climbed into the flowers to satisfy them. But such an idea would be altogether erroneous, for in their present condition they are not vegetarians at all; their true diet consists of bees' eggs, and therefore they can make no further progress with their development until they have reached the inside of a bee's cell. Hence one would have thought that, active and enterprising as they are, they would have marched straight off in search of bees' burrows, which could not fail to be found somewhere near by; but this they show no intention of doing. Their entry into their new pasture grounds is to be made in a much more romantic fashion, for they must wait till they can secure the services of some aerial steed, on whose back they may ride in triumph to their destined quarters. Here, then, is the explanation of their presence in the flowers, for some chance bee alighting on them is to become the wished-for Pegasus. Not that any bee will do—it must be one of the *Anthophoræ* described in our last paper, or else an *Andrena*; should such an insect pay the flower a visit, the little parasite is ready to scramble on to its back while it is busy rifling the flower. One can understand with what a firm grip it would seize the bee's hairs, for it is a matter of life and death, and should one chance be missed, another may not occur for a long time. This, no doubt, explains the very peculiar shape of the parasite's feet; for, in addition to the usual pair of claws at the end of each foot, there is a central pad which looks like a third claw, and gives the foot the exceptional appearance of a three-fingered hand. These little insects are also sometimes found on the bodies of flies, but whether this is due to miscalculation on their part, or to intention, cannot be stated, nor is it known what afterwards becomes of such larvæ. There is, indeed, some doubt as to the exact course followed by the *Meloë* larva after it has gained the bee's back, but as the transformations of an allied species called *Sitaris muralis*, which occasionally occurs in this country, have been fully worked out by M. Fabre, and as those of the *Meloë* are probably almost identical with these, we will now follow the fortunes of the *Sitaris* larva.

FIG. 7.—Larva of *Meloë*, much magnified. (After Newport.

The *Sitaris* is a brown beetle which is parasitic upon an *Anthophora*. Unlike those of *Meloë*, its eggs are laid at the entrance of the burrows of the bee. They are hatched in September or October, but the young larvæ, instead of at once effecting an entrance into the cells of the bees, remain where they are till the following April, taking no food all this time. The larvæ of *Meloë* seem to be equally well able to endure prolonged fasting, a very needful thing in their case, as they may have to wait long for the bee's visit which is to give them their great opportunity in life. The males of the *Anthophora* are the first to issue from the burrows in spring, and as they pass out the young parasites attach themselves to them. Soon, however, they transfer themselves to the females, and thus secure the means of entrance into the newly-made cells. When a cell has been provisioned by the careful mother with a supply of honey and pollen, an egg is laid and floats on the semi-liquid mass. Now is the chance of the *Sitaris* larva; as the egg is laid, the parasite drops upon it, apparently unnoticed by the bee, which proceeds to fasten up the cell. The parasite now has everything its own way, though care is needed, for if it were to fall off the egg it would perish in the sticky mass beneath. Sitting on its tiny raft therefore, it nibbles a hole in the egg-shell and begins to devour the contents; so small a creature is it, that this egg lasts it for a week's meals, and then it undergoes its first transformation on the empty egg-shell. No fairy's wand ever produced a more startling change than the simple process of skin-changing now effects; the active, enterprising, six-legged, slender larva becomes a fat, lethargic, almost legless grub (Fig. 8—1), whose tastes are as much revolutionized as its appearance. Animal matter not being now obtainable, a vegetable diet must perforce be substituted for it, and the grub succeeds to the inheritance of the young bee whose birth it has prevented—viz., the cell-full of semi-liquid honey; on the surface of this it floats, with its mouth buried in the mass, so that it has but to lie still and eat, and no exertions are necessary to enable it to take its fill. Its legs are therefore reduced to mere stumps, and locomotion

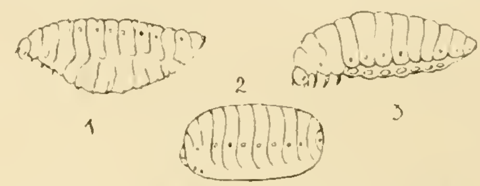


FIG. 8.—Grub of *Meloë*, showing its three stages.

becomes to it a lost art. It is so constructed that its spiracles are situated high up on its back, and therefore out of reach of the sticky sea in which it floats, and which, if it came in contact with them, would inevitably clog them up and suffocate the grub. When the honey is all consumed, a further change takes place; the grub contracts, detaching itself from its skin, which darkens and hardens, and becomes a barrel-shaped body (Fig. 8—2), within which the enclosed larva proceeds still further with its development. After a while another moult occurs, and again the form is changed, the insect appearing as a fat six-legged grub, with spiracles in the natural position (Fig. 8—3). After this it changes into an ordinary chrysalis similar to that of other beetles, and finally, after all these wonderful adventures, in the month of August the perfect beetle appears, and we are again brought back to the starting-point of the marvellous cycle of transformations. The life of an oil-beetle is very similar to this, though perhaps a trifle less complicated. From facts such as these, and the instance is by no means a solitary one, Sir John Lubbock has been led to the conclusion that, while the form of the larva of any insect is to some extent dependent upon the order to which it belongs, yet it is also greatly influenced by the external conditions to which it is exposed; in other words, that there are often changes through which an insect passes which are not to be explained by reference to the form the insect will ultimately assume, but are determined solely by the circumstances in which it then finds itself, and that therefore we may find the usual form that belongs to the larvæ of one order imitated in another, provided the circumstances of the larval life are similar, although the perfect insects produced will be totally unlike.

There yet remains for consideration a species of parasitism which affects our wild bees far more intimately than any we have yet passed in review, influencing not merely their comfort and well-being, but even their very form. The parasites in this case constitute an extraordinary family of insects which are apparently not distantly related to the *Meloidæ*; for in them we find the same transformation from an active six-legged larva to a footless grub, complicated, however, by the fact that the grubs are *internal* parasites and live, not upon the bee's food, but upon the

bee itself. The family is called *Stylopidæ*, from the chief genus *Stylops*. But we must defer the consideration of these interesting parasites till our next paper.

(*To be continued.*)

THE CLIMATE OF MARS.

By E. W. Maunder, F.R.A.S., *Assistant superintending the Solar and Spectroscopic Departments of the Royal Observatory, Greenwich.*

"The analogy between Mars and the earth is perhaps by far the greatest in the whole solar system." Such, we have been pretty frequently reminded during the past few weeks, was the opinion of Sir W. Herschel, and it may seem very presumptuous to attempt to traverse the dictum of so great an astronomer. Still, it may be worth while to look a little closely into what we know of the Red Planet, in order to ascertain if the facts of the case really bear out this view. There are two great points of difference between Mars and the earth which strike us at once as having a very important bearing on the climates of the two planets; first, the greater distance of Mars from the sun, involving its receiving a much smaller supply of light and heat, and next, the greater length of the Martian year, rendering the effect of its seasons more pronounced.

Under the first head we find that Mars only receives, on the average, three-sevenths of the light and heat which falls upon a similar area of the earth. This would be a serious matter, even if it only meant that the mean temperature of Mars lay three-sevenths of the distance from freezing-point to the mean temperature of the earth. But as it is, we must take the absolute zero of temperature as our starting-point; that is, as is well known, 273° Centigrade, giving us from 130° to 140° below zero for our result.

This will be the mean temperature of the whole planet. But Mars must have its various zones, differing from each other as much as similar zones differ on the earth, and if we confine our attention to the equator of Mars as the hottest region, we may get a somewhat closer approximation to its condition, for it is easy to find a zone on the earth, where, owing to the oblique presentation of the surface towards the sun, the light and heat incident on any square mile is but three-sevenths of that falling on an equal area at the equator. This we find in Lat. 62°. We may therefore take the difference in mean temperature which we find between Iceland and Archangel on the one hand, and Cayenne and Singapore on the other, as affording some indication of the difference between the equatorial temperatures of the two planets.

This is, of course, to put the best possible construction on the matter, for the circulation which goes on continually in air and sea tends greatly to diminish the difference between the climates of the several terrestrial zones; so that Singapore is cooler and Iceland warmer than it should be if we considered latitude alone. This second mode of approaching the problem confirms the first to this extent at least, that it gives the mean temperature of even the hottest region of Mars as below 0° C., and consequently the mean temperature for the planet as a whole must be lower still.

Nor is this all. We know that the atmosphere of Mars is far less dense than that of our own world. This is not so obvious a difference as the two already mentioned, but it is not less certain, for it is a direct conclusion from the smaller size and mass of Mars, two points which might not at first seem to have anything to do with temperature.

For the mass of Mars is only one-ninth of that of the earth, and this implies, when its smaller diameter is taken into consideration, that the force of gravity at its surface is less than two-fifths of that which prevails here. If the strength of terrestrial attraction were diminished till it only equalled the Martian attraction, our atmosphere would at once expand upwards to more than two and a half times its present extent, and the pressure on the surface of the earth (as measured by a spring balance) would be only $5\frac{1}{2}$ pounds instead of 14 pounds to the square inch, and the aneroid barometer would read $11\frac{3}{4}$ inches. So, if we assume that there is the same quantity of air above every square inch of the surface of Mars that there is for our earth, it would extend to a greater height, and exert a smaller pressure in this proportion.

This makes the condition of "our nearest neighbour" yet worse. To find a terrestrial parallel, we must not compare Archangel or even Spitzbergen with Cayenne, but must compare the summit of a mountain more than four miles high in the Arctic regions with a place at sea-level on the equator. The result would appear to be conclusive—that the mean temperature even of the equator of Mars lies far below the freezing-point.

Of course, it might be that Mars had so extensive an atmosphere that the pressure at the surface was as great as here, or even greater. If we could imagine the pressure double that which we experience, we might suppose that such an atmosphere would go far to compensate for the diminished supply of solar heat. But that would mean a total atmosphere over the unit of surface more than five times as great as we have here; and we may feel perfectly sure that such is not the case.

For, under such circumstances, we could not perceive the surface markings of the planet. Now there is a very marked gradation in the distinctness of the surface markings of the different planets. Those of the moon are perfectly distinct; no atmospheric veil hides them from us. The markings on Mars are much less apparent, and yet are tolerably distinct. It needs but a small experience to prove that the trouble in distinguishing them, down even to the very limit of telescopic minuteness, lies far more with our own atmosphere than with that of the planet. This present opposition seems to be a hopeless one for English astronomers, but that is not in the least because Mars is modestly concealing himself behind any cloud veil; it is simply that he rises so little over our horizon that we have to observe him through a great thickness of our atmosphere. Venus, again, is a far more difficult object, and such of its markings as are permanent, if any deserve that designation, are only to be made out under specially good conditions and by trained observers. Jupiter, of course, shows us a wealth of beautiful details, but it does not require a long scrutiny to see that these are purely atmospheric, and few observers will be prepared to affirm that we have ever seen any part of its real surface. It is clear, then, that so far as these four bodies are concerned, the denser and less transparent atmosphere is always found in connection with the larger planet. Mars, on this principle, should have a less atmosphere than the earth, not a greater one.

Further, Prof. Langley and Prof. Pickering have shown us that the loss of light of a ray entering our zenith is probably just about one half, the rest being partly absorbed by our atmosphere, but mostly reflected and diffused by the minute dust particles with which it is filled. It is clear, then, that an observer on Venus would have the greatest difficulty in making out the chief features of our geography, for a large proportion of the light he received from us would come from our atmosphere,

and of the light which fell on the actual surface only a small proportion would be reflected back, and this would again be reduced on its outward journey. It is scarcely likely that the difference in the reflective indices of land and water, especially when we remember how much of the former is covered by dark vegetation, would be sufficiently great to make itself noticeable. Of course, whenever our sky was covered by cloud the details of our geography would be effectively hidden. We may take it as pretty certain that our earth, so far from being as easy to observe as we find Mars, would be probably more like Venus; so that, instead of giving a greater amount of atmosphere above each square inch to Mars than we possess, there is little doubt but that there must be less. Indeed, if the total amount of the Martian atmosphere bears the same proportion to the mass of the planet as is the case here, then its density at the surface will be one-seventh of ours, corresponding to the state of things we should find if we could ascend in a balloon to a height of very nearly ten miles.

All these considerations point to the existence of intense cold upon Mars, cold distinctly below the freezing-point even for the equator; and yet observation does not seem to confirm this view.

Instead of the white icy glitter we should expect if Mars were one vast glacier, he sends to us a most conspicuously ruddy light. The intensity of its redness is, indeed, the most obvious thing about the planet. Then, again, it seems that its atmosphere contains a very appreciable amount of water vapour, a thing which we cannot reconcile with an extremely low temperature, for Dr. Huggins, observing the spectrum of the planet on February 14th, 1867, detected traces of some of the telluric bands due to water vapour, and though the observation is a very delicate one, and one may readily be deceived in the matter, I have repeated it myself on two occasions, and have little doubt as to its accuracy. If we accept these observations, then the white polar caps may be reasonably ascribed to ice or snow; indeed, as Proctor pointed out, even if we had never seen them, we could, after Dr. Huggins' observation, have confidently predicted their presence, and their gradual increase during the planet's winter, and diminution through its summer. Take the two facts together, and we come as nearly to a demonstration of the existence on Mars of land, water, ice, snow, and cloud as we could hope with our present resources, and sceptics must be able to produce a very strong case to overthrow it.

But how is it then that we do not find Mars completely ice-bound? For it clearly is not. In the summer of the southern hemisphere the white pole cap has been watched to shrink, until in 1877 the pole itself was actually clear, and the cap itself had a radius of only 130 miles; much as if the legend of "an open polar sea," so false for the earth, were true for Mars, and only the highland glaciers of some Martian Greenland were left unthawed under the summer sun.

So that the Martian summer would seem to be actually hotter than our own. And the winter, on this way of looking at it, is no colder. For the winter pole cap only reaches Lat. 40° or 45°, if indeed so far as that; that is to say, no lower than it does for terrestrial continents. Thus, if we refer to a beautiful series of drawings by Mr. Knobel, the President of the Royal Astronomical Society, published in Vol. 48 of the *Memoirs* of that body, we find that the Newton Sea was clear on February 11th, 1884, up to S. Lat. 50°, the Zollner Sea on February 26th to S. Lat. 50°, and the Maraldi Sea on March 8th up to S. Lat. 40°. These dates were all after February 3rd; the mean date between the autumnal equinox and winter solstice. Another series by Dawes, in Vol. 34 of the same publication, shows us Nasmyth Inlet in N. Lat. 43° on November 20th, 1864, the Delambre Sea in N. Lat. 55° on November 26th, and the Campani Sea in N. Lat. 65° on December 1st; the winter solstice having fallen on July 28th, and the spring equinox not coming on until January 4th, 1865; whilst the superb series by Mr. Green in Vol. 44 show the Nasmyth Inlet in N. Lat. 43° on September 10th, 1877, only a fortnight before the winter solstice, September 26th.

Now, although broad oceanic expanses are not covered here on our world to any low latitudes, none of these Martian markings can fully claim to be of that character. The Nasmyth Inlet is most strictly a "mediterranean" sea, and may fairly correspond to the Black Sea, Sea of Azov, and Caspian Sea. But the northern portion of the latter is ice-covered every winter; the Straits of Yenikalé are often bridged with ice, and navigation is stopped on the Sea of Azov. It would seem therefore that the winter of Mars is little if any severer than here. And we must remember, in addition, that we cannot discriminate between snow and cloud, and that Mars is free from *both* to as high a latitude as the earth is free from snow.

How are we to reconcile results so contradictory? The first circumstance to bear in mind is that the heat and light incident upon a planet are no measure of that effective upon it. Of that which falls upon the earth, probably nearly one half is reflected off again from the atmosphere alone, and a yet further amount is reflected from clouds. All this is ineffective in warming either air or soil, and if we could assume that all the heat falling on Mars was used directly by it, we should find it would be practically as well off as ourselves. And no doubt some approach is made to this condition, for although the determination of the *albedoes* of the respective planets has been much neglected, and such results as have been obtained are very rough, yet it is most plain that the albedo of Mars is very low, not differing much from that of white sandstone, whilst the albedoes of Jupiter and Venus are very high, and are best explained by supposing that the reflection takes place from a cloud shell. Zollner gives the moon as below Mars, the former being one-sixth, and the latter one-quarter. Chacornac rates Venus as five times the moon; whilst Seidel only credits Mars with an albedo of an eleventh. It is plain that we cannot put much reliance on the actual figures, but at the same time we may safely conclude that the reflection from Mars is almost as completely from the surface as it is with the moon, whilst from the earth, and yet more from Venus, it is chiefly from the atmosphere, either from cloud or from suspended dust, the latter agent being probably the more effective in the case of the earth, and the former with Venus.

It may be concluded then that the effective amount of heat received by the three planets is far more nearly equal than would at first appear. Mars is, therefore, much warmer than its distance from the sun would imply; but on the other hand its mean temperature is by no means so high as we might infer from the size of its polar caps. For we must not forget that we see the planet at the best advantage; the side it turns to us is the side it turns towards the sun; the parts of Mars with which we are best acquainted are its tropics at midsummer and at noon. The experience of our elevated plateaux and mountain tops shows how great may be the difference between the highest and mean temperatures where the air is rare, as it certainly is on Mars. There is nothing improbable in supposing that even if the mean temperature of its tropics is considerably below zero Centigrade, the day temperature

Three Prints from a Photograph of a Solar Prominence.

Taken by Prof. GEORGE E. HALE, on the 25th of May, 1892, with his spectro-heliograph, at the Kenwood Astro-Physical Observatory, Chicago. The upper right hand print is on the scale of the original negative. The upper left hand print is a positive copy reversed right and left.

Monochromatic Photograph of the Sun showing Spots and Faculæ.

may be consistently above it, and hence the ice and snow formed during the night would soon be melted during the day, and we should see only the indications of water and not of snow.

The sky of Mars by day is *almost* cloudless. We know this by observation, but we know it far more surely by inference; for observation at such a distance is apt to be deceptive. Here a most fruitful source of condensation is the lowering of temperature, consequent on the expansion which rising currents of air experience as they attain a greater height. On Mars this cause is far less effective. For that a body of air may expand to double its former volume, it must rise nearly nine miles instead of three and a half, and it will take more than six times as long to do so. Nor is this all. Its temperature would not be lowered so much by such an expansion, for less work would be done, and even an equal loss of temperature would make a less difference to the power of the air to hold water vapour. For whilst the boiling point of water would be lowered some 24° Cent. by an ascent to the height of half-density here, it would on Mars (assuming an atmosphere proportional to the mass) be lowered only 13°. It would be fairer, however, to compare the effects of an equal motion, for the nine miles rise, which would lower the boiling point on Mars by but 13°, would lower it on the earth by 85°, or six and a half times as much.

It is easy to see how it comes to pass that the spectrum of Mars affords evidence of the presence of water vapour. For evaporation would be easy and rapid, the boiling point of water being (on the assumption made before of an atmosphere proportioned to the mass) 46° Cent. instead of 100°; whilst condensation would be difficult and slow. The tendency would be during the day for the atmosphere to become as fully laden with water vapour as it would hold. At night condensation would indeed set in, but the formation of a continuous cloud canopy would probably interfere to check radiation, and would prevent the temperature falling as low as we should suppose. The traces of this night cloud canopy are probably seen in the white rim always observed on the east and west limbs, and always somewhat broader on the side emerging from darkness; for both mornings and evenings are always cloudy on Mars, but more especially the mornings. The low density of the atmosphere would prevent differences of pressure being set up at all comparable to those we know here; the feeble gravity of the planet would make the movements in response to such differences far more languid. There are no hurricanes in Mars. The clouds will be cirri, not cumuli, hoar frost will be far more common than snow, and the currents, such as they are, will not bring moisture enough to the pole in winter to cover it with snow to any great depth, so that the succeeding summer may well be able to melt it nearly all away.

If these considerations be correct, it is Venus and not Mars which bears the greatest analogy to our own planet: it is more nearly equal to the earth in size and mass, and hence I would suggest its meteorological conditions are more nearly similar. Mars with the smaller amount of heat that it receives, its thin atmosphere, and sluggish meteorology will, in spite of our knowing the configuration of its surface so well, present more differences than analogies. And what we do see is probably deceptive: the ruddy glow and apparently open seas of the part turned towards us are very likely perfectly consistent with the part which we do not see being bound in ice, or perhaps we should rather say in frost.

Remarks by A. C. Ranyard.

[I do not feel as certain as my friend Mr. Maunder that the atmosphere of Mars is less dense than our own; but I agree as to the strong evidence tending to show that the Martian atmosphere is more transparent than the earth's, and with Mr. Maunder's conclusion that it must be less dust-laden. This seems to follow naturally from the more languid character of the storms which Mr. Maunder has so ingeniously shown to be a natural consequence of the feeble gravity at the Martian surface. Most of the larger dust particles in the earth's atmosphere are suspended within a mile or two of the surface. During the last few years, it has been shown that the dusty state of the air is intimately connected with cloud formation. Thus on Mars we never observe white seas of cloud such as float so frequently over the earth's surface, and which must obscure its oceans and continents with a snow-white sheet, ever drifting and changing as it forms or dissolves.

The intense polarization of the sky, as seen from a mountain-top, proves that our upper air is full of very fine dust, which is possibly partly of terrestrial origin and partly derived from the *débris* of meteors which have been driven into vapour in the upper air. But it does not even follow that the number of meteors which plunge into the atmosphere of Mars correspond with the number met by the earth. The large majority of meteors which we encounter evidently belong to elliptic streams, and—whether we assume with Proctor and Sir R. Ball that such streams had a terrestrial origin or, as I think more probable, that they represent the remains of comets captured by the earth—it seems likely that the earth would encounter more of such closed streams of meteors than Mars; for, on the one theory, Mars, during its sun-like stage, must have been a feeble little centre of disruptive action as compared with the earth, and, on the other theory, Mars' power of capturing comets must be feeble compared with the earth's.

It is evident that the red colour of Mars is not due to the absorption of its atmosphere, for, as Webb remarks, its polar snows never have a ruddy tinge, even on the limb. We see no evening and sunrise tints on Mars. When it presents the gibbous form, the terminator where twilight effects should be visible seems somewhat more sharp than the limb or outer edge of the planet, which always appears rather whiter and more misty, as Mr. Maunder remarks, than the rest of the planet. On the earth, twilight lasts till the sun has sunk about 18° below the horizon, and if there were such a band of soft degrading light on Mars, it ought to be easily recognizable when Mars presents its most gibbous form. But this absence of detectable twilight is quite consistent with a very dense Martian atmosphere, if the air were very clear and free from dust.]

ON SOME RECENT ADVANCES IN THE STUDY OF SOLAR PROMINENCES AND FACULÆ.

By A. C. Ranyard.

It may be said that till recently photography has lagged behind direct observation in the study of solar phenomena. The first great advance was made by Janssen, who, more than ten years ago, photographed details of sunspots and of the sun's surface which the eye could not recognize. Prof. C. A. Young, some years ago, obtained a few photographs of bright solar prominences, but they showed very little structure compared with the wonderful detail which can be seen with the spectroscope around the sun's limb. There is no lack of light for the study of solar structures, but the very abundance of light and heat increases the difficulties presented by the troubled ocean of atmosphere,

through which we look up at all the phenomena of the outer universe.

The bright light from the body of the sun lights up the dust in the atmosphere, and forms a luminous veil which is drawn over the whole sky and hides from us, under all ordinary circumstances, the brilliant prominences and fainter corona which surrounds the sun. At first it was only when the dust veil was rendered transparent by being plunged into the shadow of the moon that the veil was, as it were, rent for us, and for a few moments, during a total eclipse, we were permitted to look upon the wonderful structures about the sun. Then came the method of stretching out into a spectrum the white light dispersed by the dust veil, and observing upon the band of colour the unstretched coloured images of the gaseous structures behind the white veil. But, under all ordinary conditions, when the coloured images are bright enough to photograph, only a narrow slit can be looked through, showing a very narrow slice of the solar structure.

The ingenious advance made by Prof. Hale of Chicago, which has enabled him to photograph large prominences and monochromatic images of the whole disc of the sun, consists in employing a moving slit which is carried across the prominence intended to be photographed, or across the whole image of the sun, by a clepsydra, while another slit is evenly moved at a corresponding rate just in front of a photographic plate, so that the slit only allows one coloured image to fall upon the sensitive plate and screens it from the action of all other coloured images, and a photograph printed by monochromatic light is obtained.

The apparatus at first used consisted of a cylinder in a closed box, at the eye end of the observing telescope of a large diffraction spectroscope. The axis of the cylinder was parallel to the lines in the spectrum, and the cylinder could be rotated at a uniform rate by a small clepsydra driven by a supply of water mixed with spirits of wine, so as to avoid the chance of freezing in cold weather. A strip of flexible celluloid photographic film on the circumference of the cylinder was slowly moved in the plane of dispersion behind a narrow slit at the focus of the observing telescope. The diffraction grating was rotated until the K line in the fourth order spectrum passed through the slit and fell upon the sensitive film. By changing the rate of the driving clock of the telescope, the sun's image was made to drift slowly across the first slit of the spectroscope, while the film rotated at the proper speed. In this way, if the motion of the cylinder is properly proportioned to the motion of the driving clock, a circular image of the sun can be built up. In the image of the sun shown on the lower half of our plate the two motions were not properly proportioned, and consequently the image is elliptical. Our plate is by no means a satisfactory one. The glass photograph kindly sent by Prof. Hale as a specimen of his work, shows two bands of bright faculæ running across the sun's disc in the region where spots are most frequently found.

The faculæ, shown in this photograph, all correspond to the bright monochromatic solar image K, though no doubt they shine with other light as well, but the K line is generally the most active photographically in the spectra of the prominences, to which the faculæ are evidently allied. Under ordinary circumstances, when examining the sun with a telescope, faculæ of much less extent are seen, and they are only clearly recognizable near to the sun's limb. The photographs made by the new method reveal the fact that they do not merely equal the spots in area but that they are far more extensive—in fact the spots sink into relative insignificance beside the faculæ which surround them.

Prof. Hale has recently succeeded in making photographs in which the faculæ, spots, chromosphere, and prominences are all shown on a single plate in their proper relative positions. This cannot be done with Prof. Hale's instrument in a single exposure, as the time required to bring out the prominences is much too long for the faculæ. A diaphragm covering the sun's image at the focus of the equatorial is therefore employed, and the slits are made to move across at the speed required for the prominences. At the end of the stroke the diaphragm is removed, and the slits are made to move back over the image at a much higher speed by adjusting the valves of the clepsydra. An image of the sun's surface is thus formed on the plate exactly within the image of the chromosphere formed during the whole exposure, and the whole operation can be completed in a minute. One great advantage of the photographic method is that sudden and short-lived eruptions, with which all solar observers are familiar, can be studied and measured at leisure.

The upper pictures on our plate represent a fine prominence photographed by Prof. Hale in May last. Much of the faint and delicate detail visible in the glass picture sent by Prof. Hale is lost in our copy, but our readers will no doubt be able to recognize several dark tree-like forms cutting out the light of the bright prominence. These, no doubt, correspond to a cooler group of prominences situated between us and the larger bright prominence, and they are seen reversed on the bright background.

We heartily congratulate Prof. Hale, and wish him still further success in the fruitful line of research which he has made for himself.

Letters.

[The Editor does not hold himself responsible for the opinions or statements of correspondents.]

THE NOVA AURIGÆ.

Extract from a letter from the Rev. T. E. ESPIN, *dated Towlaw, Darlington, 22nd August,* 1892.

. . . I had a letter from Mr. Henry Corder yesterday telling me that the Nova was again visible. I looked it up last night, and found it 9·2 mag., white or yellowish white. The spectrum has changed to a monochromatic one, all the star's light being concentrated in one intensely bright line, which is probably λ 500·0. The Nova, like the Cygnus one, seems to have become a planetary nebula. . . .—T. E. ESPIN.

[The changes observed in the spectrum of the Nova Cygni as it vanished seem to show that the nebular type of spectrum was not the last that it presented. Dr. Copeland and Lord Crawford at Dunecht noted the single greenish line of the nebular spectrum, λ 500 ±, as the only line in the spectrum of the Nova on 2nd September, 1877*. Some three years afterwards its spectrum was examined with the 15-inch refractor at Harvard College Observatory, and found to correspond with the spectrum of an ordinary star. Dr. Copeland was, in 1881, unable to see any trace of light with the spectroscope at Dunecht, which is sufficient proof that its spectrum could not then have been monochromatic. By 1885 it had decreased to a star of the 15th magnitude. Dr. J. G. Lohse, who examined it with Mr. Wigglesworth's 15½-inch refractor near Scarborough, was unable to detect any spectrum when examined with the spectroscope. It then appeared bluish in colour and nebulous. Thus the monochromatic nebular

* *Copernicus*, Vol. II., p. 108.

type of spectrum seems to have been presented only during a comparatively short stage in the development of this remarkable nebulous variable.

The Auriga Nova is evidently not diminishing uniformly in brightness. According to an article by Dr. Copeland, published in the August number of Professor Hale's *Journal of Astronomy and Astro-Physics*, p. 600, the Nova exhibited continual and irregular changes of brightness throughout the month of February. On the 20th of March it had dropped to the 9·1 magnitude, and by March 28th to the 11·9 magnitude.* Its present estimated magnitude, 9·2, would indicate a rise of more than $2\frac{1}{2}$ magnitudes. These irregular changes in brightness and in spectrum favour the theory, suggested in the June number of Knowledge, that the light of the Nova is due to its passage through an irregular nebula rather than to a single collision or near approach of two stars moving in nearly opposite directions, as suggested by Dr. Huggins.—A. C. Ranyard.]

To the Editor of Knowledge.

Sir,—An active aurora of great brilliancy was visible here on the evening of Friday last from 9 till 10 p.m.

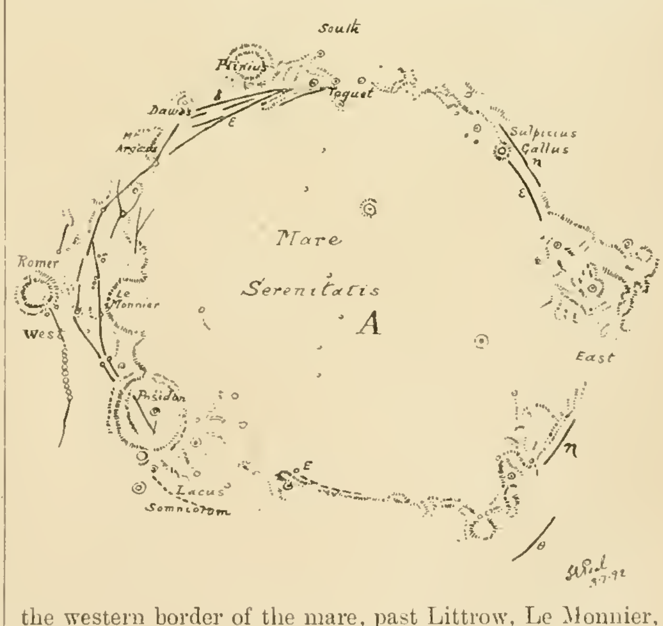
The whole of the sky, from N.W. to N.E. and from horizon to zenith, was filled with a mass of streamers. The light was sufficient for reading moderately large type. The streamers and rays were projected from the upper edge of a low arch of dark-coloured vapour resting on the northern horizon. The space occupied by the points of the streamers covered the constellations Ursa Major on the west and Cassiopœia on the east, and the intermediate region. Among the brilliant sheaf of white streamers, an occasional dark ray shot upwards from the generating arch. J. Lloyd Bozward.

Worcester, 17th August, 1892.

ARE THE LUNAR CLEFTS "RIVER BEDS," OR FRACTURES OF THE CRUST?

To the Editor of Knowledge.

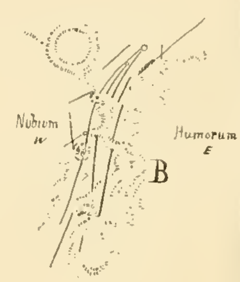
Dear Sir,—One of the most remarkable geological discoveries of modern times is the fact that the floors of our great oceans are vast areas of slow permanent subsidence, by the steady sinking in of which, through shrinkage due to secular cooling, the persistent degradation of the continents is counteracted. Mr. J. Murray tells us that "the result of many lines of investigation seems to show that in the abysmal regions we have the most permanent areas of the earth's surface"; and M. Faye points out that "under the oceans the globe cools down more rapidly and to a greater depth than beneath the surface of the continents—at a depth of 4000 metres the ocean will still have a temperature not remote from 0° C., while at a similar depth beneath the earth's crust the temperature will be not far from 150° C." This greater relative density of the crust beneath the oceans compared to the continents has been amply proved by a long series of pendulum experiments, and is accepted as an axiom. According to Mr. Murray, "the general aspect of the abysmal regions must be that of vast undulating plains, interrupted here and there by huge volcanic cones," which rising to the surface occasionally, form the oceanic (volcanic) islands. Lying at an average depth of three miles, these areas of permanent subsidence cover $\frac{7}{16}$ths of the globe's surface, and the proofs that they have never formed part of a continental surface are very clear in several ways, one being the entire absence of stratified rock fragments (sandstones, &c.) from the ejecta of oceanic volcanoes, while so common in those existing on continents. The area of the shallow and enclosed seas is put at $\frac{3}{16}$ths of the earth's surface, that of the land being $\frac{5}{16}$ths, the mean elevation of the latter being 900 feet. Both of these contrast strongly with the abysmal plains, inasmuch as they are marked by great inequalities of level, the fluctuations being revealed in the strata. The fact for us to note specially in the foregoing is, that the slow shrinkage of our globe—due to secular cooling—is mainly taken up in and by the steady subsidence of our ocean floors, which are the coldest, densest, and heaviest portions of the earth's crust. This slow, persistent, and invariable sinking in of the sea bottoms is such a momentous feature in terrestrial surfacing that we may well keep it in view when endeavouring to solve the mystery of lunar details. The question arises, Can we trace on the moon any evidence of subsidence in the marea, or seas? To begin with, we may legitimately assume that whether the surfaces we see are composed of sand or alluvium—the sea beds laid bare—or whether they are the actual seas, solidified and floe-covered, the absence of vegetation, and of all atmospheric and pluvial erosion, would render all fractures of the crust due to mareal subsidence both more permanent and conspicuous than they would be on our earth. We may also assume beforehand that repeated (if slow) subsidence of any large circular area, such as Mare Serenitatis, would eventually result in the formation of a series of marginal tangential fissures, visible or invisible. Now, on examining the borders of this sea for evidences of subsidence, we at once find the most remarkable confirmation of the above. Beginning at the southern margin near Plinius, we find on the great incline a triple system of tangential clefts, one of which, δ, extends in a straight line from Taquet B to Dawes, while another, ε, of still greater length, runs on the north towards Mount Argæus. From thence, an almost continuous series of clefts runs tangentially around



the western border of the mare, past Littrow, Le Monnier, and Chacornac, to, and across the walled plain Posidon, on the north-west, taken up faintly in crossing Lacus Somniorum, and at the foot of the cliffs east of E, in longitude 20′ W.—the northern coast line. Again, on the eastern border at N, we have another cleft, tangentially placed, like two others, N and E, at the Sulpicius Gallus on the southern margin, thus completing the circuit of the mare,

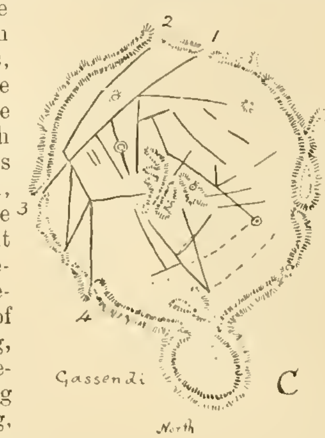
* According to Professor E. C. Pickering's measures, it had dropped to the 14·3 magnitude on April 13th. See *Astronomy and Astro-Physics*, May, 1892, p. 417.

as per diagram A. That such an arrangement of "clefts" as the above could have ever been "river beds," or *due* to river action, is simply incredible, the more so, as many of them traverse inclines athwart the drainage slopes, the "gape" of the clefts being precisely what it should appear if they were vast fissures, or marginal fractures of the crust, due to subsidence of the adjoining mare. Clearly, we are not justified in ignoring all this evidence, derived from the arrangement of the "clefts" surrounding Serenitatis. It is distinct and positive evidence against the view that they are due to river action, and equally clear that they are marginal fractures of the crust, due to the subsidence of the enclosed sea; in other words, a proof that the subsidence of sea bottoms is a lunar as well as a terrestrial phenomena. This type of arrangement is seen elsewhere, as in Mare Humorum, and may be called type A. Another type of arrangement, B, is at times seen, where two



mares are divided by what appears to be a shoal, as between Humorum and Nubium, over and along which a large group runs more or less parallel to each other. In this case (see diagram B) the group of clefts runs parallel to and over what would be the "water parting" which divides the two seas, an arrangement directly opposed to the view that they are "river beds," but precisely that which we should naturally expect if the clefts were due to subsidence of the marea on each side, east and west. A third type of arrangement, C, is where we see the clefts conspicuously forked and crossed, as in the walled plain Gassendi, at Ramsden, Triesnecker, &c. In these cases it is equally difficult to understand how such clefts could be regarded as due to river action; not only do they cross each other in all directions, in a manner quite opposed to our experience of terrestrial river beds, but in the case of Gassendi the entire system lies within the ring of the walled plain. If, however, there has been an extensive subsidence, not only of Mare Humorum to the south, but also of the mare on the north-west and west, and of the ring itself, on to and around a rigid centre, or cluster of peaks, such a complicated series of fractures will be at once intelligible. In the case of Gassendi, indeed, there is remarkably good internal evidence that this view is correct. It will be seen on reference to diagram C, drawn mainly from Neison's map of Gassendi, p. 337 of his "Moon," that the ring of this formation is gapped in two places on the southern border 1 and 2, and that each of these gaps, or passes, coincides with the extremity of a cleft on the floor inside; also that the ring is gapped in two places, 3 and 4, on the western border, each gap again coinciding with the extremity of a cleft on

the floor. This peculiarity was observed by Neison, who says at p. 342, "Some connection appears also to exist between the rill system and the peculiar passes in the walls of the formation." The connection referred to seems to be this, that the mareal subsidence and displacement to the south-west and west, which opened the crevasses or clefts on the floor, in line with 1, 2, 3 and 4, at the same time *necessarily* gapped the ring at these same places. This remarkable coincidence, therefore, of the extremities of clefts with gaps in the ring, amounts almost to a demonstration that adjoining mareal subsidence, involving, as it necessarily must, some little lateral displacement, has by the same operation both cleft the floor and gapped the ring.



Thus in the foregoing, while we have seen in A, B and C three quite distinct types of arrangement of the "clefts," they have in all cases, on examination, told the same tale—*i.e.*, that instead of being river beds, they are fractures of the crust or fissures, due to adjoining mareal subsidence. This view is indeed confirmed on a large scale by the vast series of clefts, extending from Hyginus, past Sabine, Censorinus, and Capella, as far as Goclenius, which border the Marea Vaporum, Tranquilitatis, and Fœcunditatis, and is also seen on a small scale in many places. But there are occasionally instances wherein it is not so easy to adduce "mareal subsidence" as a cause for the cleft, yet easy to show that it cannot have been a river bed. The great cleft of Sirsalis, for instance, which runs in a fairly straight line, crosses hills, valleys, craters, and mountain ranges quite impartially for some 400 miles, a series of feats which no terrestrial river is ever likely to accomplish.

But quite apart from the evidence based on the arrangement of clefts, we have in their forms alone fairly good proof that they are fractures of the crust and not river beds. As Proctor points out in his "Moon," page 176, they extend "with perfect straightness for long distances and changing direction (if at all) suddenly, thereafter continuing their course in a straight line." This notorious peculiarity is utterly opposed to all our experiences of terrestrial rivers, the sinuosities of which are well known. But the straightness and angularity is what we should naturally expect to see if clefts are fractures in a tolerably thick, brittle, and semi-rigid crust, induced by the subsidence of adjacent areas—they are not at all the features we should expect to see either in a thin crust, or one of open friable nature like sand or alluvium; in the latter we should certainly see a maze of short, irregular reticulations, instead of the great length, straightness, and angularity.

But a singularly conclusive and beautiful proof that the "clefts" are fractures, or fissures in the crust, which pass through an outer shell, down to moister and warmer substrata, is seen in their frequent structural association with small craterlets and crater cones. These are so often seen at the angles, or junctions of clefts, that they are obviously due to some deeply-seated cause for unusual dislocation of the strata. At times we see, as in Hyginus, three or four craters isolated along the cleft, in other cases they may lie close together, the well-known "crater row,"

as many as eight or ten all touching each other, as near "Romer." These little hills, having orifices on the summits, lie directly over the cleft. The so-called "river bed" does not avoid them and go round the base, hence the proof that the cleft was made first, and the craterlet, or cone, arose *on it* afterwards. There can be no doubt, therefore, that these craters and cones are piled-up formations, and hence that their invariably snow-white material has been derived from beneath the surface—through the cleft—at isolated localized passages. If these little piled cones were occasionally coloured reddish, light or dark brown, or dark grey, we might suspect they were formed of volcanic ejecta; their absolutely uniform whiteness, however, all over the globe, whether at the poles or equator, whether on mountain regions or marea, forces us to the conclusion that they are all formed of snow, exhaled as aqueous vapour through orifices along the clefts, which here and there pass down to warmer and moister substrata. Thus, while this frequent structural association of craterlets and crater rows, with clefts, is intelligible, if we look on the latter as profound fissures in the crust, it is utterly unintelligible if they are viewed as water-courses.

On the moon, as on our earth, there is yet evidently a vast store of internal heat; as this is slowly dissipated, and induces internal contraction, the lunar marea, *like our terrestrial sea bottoms*, slowly sink in, fracturing the outer crust, as we have seen, by doing so, in several different ways, as in A, B, and C; showing us also that the outer "crust" of the globe is of a somewhat brittle nature and of considerable thickness; and, lastly, that the dissipation of heat-energy is largely effected all over the surface, by the exudation of aqueous vapour through pores or passages, over and around which it piles as minute cones or "craterlets" of snow.

From the equator to the poles they are seen in countless thousands as a late or recent feature, and one of them quite naturally arising in Linné would explain the change seen therein, and solve that celebrated mystery.

Sibsagar, Assam. S. E. Peal.

[The evidence brought forward by Mr. Peal with regard to the general subsidence of the great lunar marea seems to me conclusive. The passage from Mr. Neison's book on the Moon, quoted in the June number of Knowledge (p. 115), does not refer to the rills or clefts as river beds, but only speaks of them as bearing "some resemblance" to water-courses, and as frequently commencing "at the end of a system of branched valleys leading from a highland." Mr. Neison says (p. 72), "With regard to the true nature of these rills or clefts absolutely nothing is known, whilst they are too delicate objects to allow much, if any, of the detail of their formation to be made out. It has been supposed they are cracks or fractures in the lunar surface; but their intersection and general conditions of existence seem quite inconsistent with such a supposition, more especially in their behaviour with reference to the various formations they pass through, round, or over. In many points they bear some resemblance to the dried beds of lunar water-courses or rivers, but in many features do not seem in accord with such an origin, though perhaps it presents the most feasible explanation of their nature of all; but their true nature will not be ascertained until they have been made the subject of a searching examination with a powerful telescope of the highest excellence, and thus details of the method of their construction have been obtained. Perhaps, unlike the terrestrial river beds, these rills may have arisen independently, but have served afterwards the purpose of river beds; their connection with the system of delicate valleys renders such a view somewhat probable. Thus many of these rills commence at the end of a system of branched valleys leading from a highland, whilst others can be detected winding along the bottom of extensive valley regions. At other times they appear however, entirely independent of such formations."]

THE OLDEST FISHES AND THEIR FINS.

By R. Lydekker, B.A.Cantab.

In the article on "Mail-clad Animals," published some time ago in Knowledge, it was shown how the fishes of the more ancient periods of the earth's history were frequently characterized by having their bodies protected by a coat of armour, and also how this armour has been lost by most of their modern descendants. At the same time it was mentioned that a few of these mail-clad fishes, like the gar-pike of the rivers of North America, and the many-finned bichir (*Polypterus*) (Fig. 1) of the

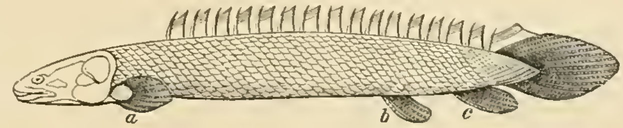


Fig. 1.—The Bichir.

Upper Nile, still linger on, as if for the purpose of showing us what their ancestors were like.

In addition, however, to their armour, and its gradual loss with the advance of time, there are many other points of view from which these ancient fishes are of more than ordinary interest, and we accordingly propose in this article to consider the curious modifications which have taken place in the structure of their fins as we ascend in the geological scale. We shall, moreover, be led to notice briefly one of the most remarkable types of fossil fish teeth found in the older secondary rocks, since it proves that one of our living fishes is the oldest kind of vertebrate now inhabiting the earth.

Before going further, we must mention that existing fishes have been divided into several main groups, distinguished from one another by structural peculiarities. One such group includes the sharks and rays, characterized by their cell-like gills and scaleless bodies. Then we have the smaller group of lung-fishes, now represented by the baramunda, of Queensland (figured in the article on "Mail-clad Animals"), and the mud-fishes of the rivers of Africa and South America, all of which can breathe either by gills or by lungs. Another group is formed by the so-called ganoid fishes (Fig. 1), many of which have the bony armour already mentioned; while the great majority of the fishes of the present day, although nearly related to these ancient ganoids, have been generally separated as a distinct group, under the title of bony fishes. That name they take from the circumstance that their skeletons are fully ossified, and do not partake of the cartilaginous nature of those of a shark or a ganoid.

Now if we look at the paired fins (or those which correspond with our own limbs) of any ordinary bony fish, such as the perch (Fig. 2), we shall see that they are formed of a number of bony rays, starting from a single point of origin, and thence spreading out in a fan-like manner. We shall also not fail to observe that the tail of such a fish has a very similar kind of structure, likewise consisting of bony rays, symmetrically arranged, and starting from a curved line where the scales suddenly stop. We may also see from the figure of the skeleton of such a fish that the backbone likewise stops suddenly where the

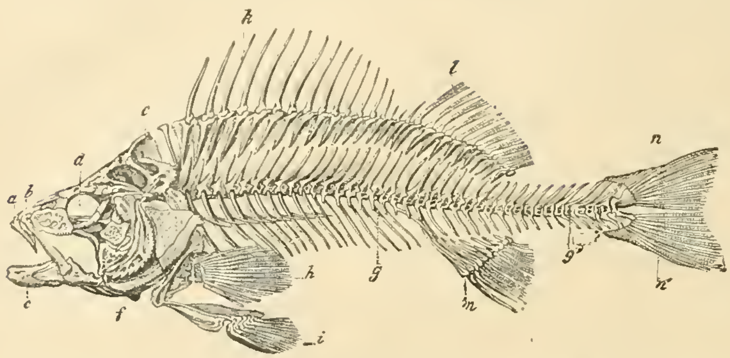


FIG. 2.– Skeleton of the Perch. *a—c*, jaws; *d*, eye; *e f*, portions of skull; *g g'*, backbone; *h*, pectoral fin; *i*, pelvic fin; *k l*, first and second back-fins; *m*, anal fin; *n n'*, tail; *h* and *i* are the paired fins.

tail begins, and that the rays of the latter start from the expanded end of the backbone itself.

The same general type of structure obtains in the paired fins of some of the later ganoids, such as the living sturgeons, as well as certain extinct forms, and it is also universally present in all the living sharks and rays. If, however, we were to go to the Natural History Museum and examine the baramunda of the rivers of Queensland, or the gar-pike (*Lepidosteus*) of those of North America, or the bichir (*Polypterus*) of the Upper Nile and the rivers of western equatorial Africa, we should find a totally different structure obtaining in the paired fins. In all these three fishes (of which, be it carefully noted, the first is a representative of the lung-fishes, while the second and third are ganoids) the first pair, or pectoral fins, as is well shown in the bichir represented in Fig. 1, are seen to have a long central lobe running for some distance up the middle of the fin, and completely covered with scales, while the rays of these fins form a kind of fringe, radiating on all sides from the central lobe, the skeleton of a fin of this type being shown in Fig. 3.

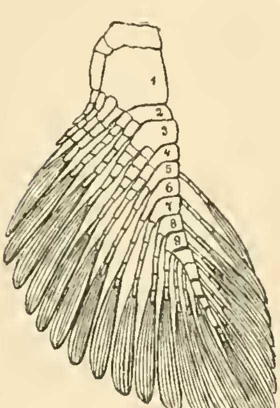


FIG. 3.—Skeleton of pectoral fin of an extinct Shark. (After Fritsch.)

From this it will be seen that such a fin consists internally of a long cartilaginous axis, composed of a number of joints (1-9), and that from one or both sides of such joints there are given off obliquely other smaller jointed rods terminating in the fine rays forming the free edges of the fins. How totally different in construction is this kind of fin from that of the perch will be manifest from our description, and a comparison of Fig. 3 with Fig. 2. Fishes with paired fins like those of the perch may be well termed fan-finned fishes; while those with fins of the type represented in Fig. 4 may be known as the fringe-finned fishes.

At the present day the only fringe-finned fishes are the baramunda and the African and American mud-fishes (which, as we have said, are the sole living representatives of the lung-fishes), together with the bichir and an allied species, and the gar-pike, which, as we have seen, belong to the ganoids. All the oldest ganoids, such as those found in the old red sandstone of Scotland, of which an example is figured in the article on "Mail-clad Animals," are, however, likewise of the fringe-finned type; and since a gradual passage from these primeval ganoids can be traced through later ganoids found in the upper palæozoic and secondary rocks to the bony fishes so characteristic of our present seas and rivers, there can be no manner of doubt but that the fringe-finned type is the most ancient one, which has gradually become modified into the modern fan-finned form.

The evidence that the fringe-finned type is the oldest does not, however, stop here; for, curiously enough, not only had the early ganoids and lung-fishes this kind of fin, but the same type likewise obtained in the primeval sharks. The fin-skeleton represented in Fig. 3 belongs indeed to a member of the same group of sharks as does the species of which the entire skeleton is shown in Fig. 4, where the rod-like axis in both pairs of fins is distinctly seen. Now we have already mentioned that modern sharks and rays have fan-like fins; and it is, therefore, clear that both in the sharks and in the compound group represented by the ganoid and the bony fishes there has been an independent transition from the fringe-finned to the fan-finned type. On the other hand, in the lung-fishes, which, as we shall see shortly, are a very ancient race, the fringe-finned structure has been preserved without alteration throughout countless ages.

We are still unacquainted with the habits of some of the living fringe-finned fishes, but at least the lung-fishes are species living partially buried in the mud, and are evidently not adapted for swimming rapidly. On the other hand, the fan-finned modern fishes, whether they be sharks or whether they be bony fishes, are generally adapted for rapid motion in the water. Any person who has watched a bowl of gold-fish will not have failed to notice the incessant and rapid motion of their film-like fins, and it is quite evident that this rapid motion could only be produced by fins of the fan-like structure. The fringe-fins are, indeed, more like clumsy paddles, capable only of comparatively slow and steady motions; such movements being sufficient for fishes protected either by the bony armour of the ganoids, or by the spines with which the early sharks (Fig. 4) were armed. The fan-like fin is therefore obviously the most specialized type of structure, and as the ganoids in their advance towards the bony fishes gradually acquired this fan-like fin, and with it, we may presume, increased speed, it was essential that their enemies the sharks should follow suit in order to be able to catch their prey. This would appear to be a sufficient reason for the attainment of the fan-like structure of fin in both these groups of fishes. It is, however, very remarkable that this structure of the fins having been independently developed in the two groups should have become so alike as it is. On the other hand, the lung-fishes, together with the gar-pike and the bichir, never having had occasion to abandon the mud-loving and sluggish habits of their palæozoic ancestors, have fortunately preserved for us intact the old fringe-finned type of swimming organs.

It is not, however, in regard to these paired fins alone that fishes show a modification from a long, jointed, axial structure, to one which stops suddenly in an expanded termination, from which arises a fan-shaped arrangement of rays, the same kind of modification, although far less generally, having also taken place among fishes in the structure of their tails.

Thus, in all the primeval fishes the backbone (as shown in Fig. 4) is continued right to the very end of the tail, where it terminates in a point. On either side of the

backbone are fringes of fin-rays, so that (as shown in Fig. 1) in scaled fishes the scaly part of the tail is continued nearly to its extremity. This type of tail is therefore exactly similar in structure to the fringe-finned type of fin, and may be similarly known as the fringe-tailed type. In some fringe-tailed fishes the fringes on either side of the tail (as in Fig. 1) are of nearly equal depth. In other instances, however, the fringe of rays on the lower side is somewhat deeper than that on the upper; and a further development of this inequality results in the partially-forked tail of the sharks, where the end of the backbone is bent upwards into the upper and longer half of the tail, the lower lobe of which is formed solely of rays. Sharks and lung-fishes have, indeed, never advanced beyond one or other of these two modifications of the fringe-tailed type. On the other hand, the compound group, including the ganoids and the bony fishes, was by no means satisfied with the primitive arrangement of matters. Starting from a fish of the fringe-tailed type like the one represented in Fig. 1, we may trace a gradual shortening of the central part of the tail-fin, accompanied by an increasing development of the rays on its lower side, until we finally reach the completely-forked tail of the perch (Fig. 2), in which, as we have seen, the backbone stops short of the fin-rays, and ends in an expanded extremity from which these rays are given off in a fan-like manner. The bony fishes have, therefore, not only succeeded in developing the fringed fins of their ancestral ganoids into those of a fan-like type, but have likewise effected a precisely similar modification in the structure of their tails. That the fan-like tail of the perch is an improvement as a steering organ upon the fringed tail of the early ganoids there can be no doubt; and it is such an organ which alone could regulate the movements of the bony fishes in the delicate manner observable in a bowl of gold-fish. To the sharks, however, whose movements largely partake of vigorous rushes, it is probable that the forked modification of the fringe-tailed type is more advantageous than would have been a tail of the fan type.

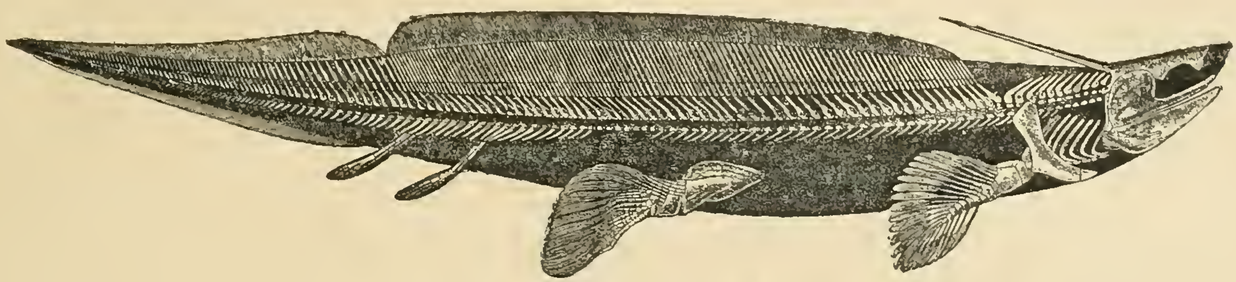


Fig. 4.—Skeleton of an extinct Shark, greatly reduced. (After Fritsch.)

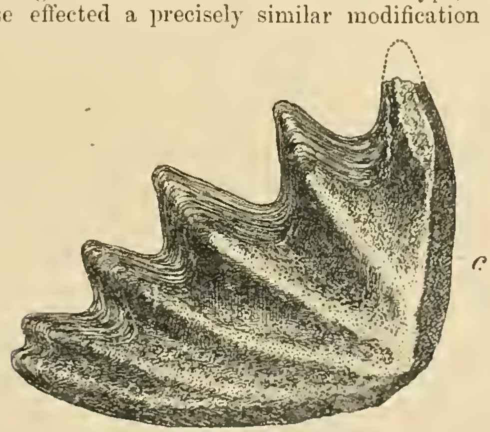


Fig. 5.—Right upper tooth of an extinct lung-fish *(Ceratodus)*. *c*. Point of contact with opposite tooth. (After Teller.)

Before leaving the subject of the tails of fishes, we cannot forbear to mention that the alteration from the fringed type, with its long central axis formed by the backbone, from each joint of which springs a pair of rays, to the fan-like type, with all the rays arising together from a blunt and shortened backbone, is precisely paralleled among birds. Thus the ancient birds of the jurassic rocks, known under the name of *Archæopteryx*, had their backbone prolonged into a long tail, from each joint of which there arose a pair of feathers. Such a tail was therefore essentially a fringed one. In modern birds, however, as we all know, the backbone extends but a short distance behind the haunch-bone, and then extends into a ploughshare-like bone, from which the feathers of the tail expand in a fan-like manner, very similar to the rays of the tail of a bony fish; with the exception that, whereas in fishes the fan is placed vertically, in birds it is expanded horizontally. In many groups of animals besides these we have mentioned it appears, indeed, that long tails have gone out of fashion, as being useless encumbrances. We have instances of this in the higher apes and bats, in bears, in guinea-pigs, and in the more specialized kinds of flying-dragons, or pterodactyles, described in an earlier number of this magazine.

Having said this much as to the fins of the ancient fishes, we may conclude our article by giving some particulars relating to the geological history of the baramunda, which, from the structure of its fins, we have already seen reason to regard as one of the most ancient types of existing fishes. For a number of years there have been known from the triassic, or lowest secondary strata of Europe, fish-teeth of the peculiar type of the one represented in Fig. 5. The remarkable horn-like form of the ridges on these teeth suggested the name of *Ceratodus* for the otherwise unknown fish to which they pertained. Nothing more was discovered as to the nature of this problematical fish, and it was even doubtful in what position these teeth were placed in the mouth, or how many of them there were in each jaw. Thus matters stood till some twenty years ago, when naturalists were startled by hearing that a large fish had been discovered living in the rivers of Queensland, which had teeth like these problematical fossils. This fish was no other than the baramunda, which, as we have seen, is one of the few living species still retaining the ancient fringed fins. It was found that the baramunda had one tooth on either side of each jaw, placed in the same position as the figured example; and it was naturally considered that the living fish belonged to the same genus as the *Ceratodus* of the trias. Here, then, we are confronted by the remarkable circumstance that a kind of fish first made known to

us by fossil teeth from the very lowest secondary strata of Europe, was actually represented by one apparently belonging to the same genus in the rivers of Australia. It is true, indeed, that a recent discovery has shown us that the living baramunda differs slightly in the structure of its skull from the fossil *Ceratodus,* and that the teeth of the opposite sides of the jaws were not in actual contact with one another, as were those of the latter. Although these points of difference are considered sufficient to warrant us in regarding the baramunda as not actually belonging to the same genus as the fossil teeth, yet this does not detract from the extreme interest of this fish as being by far the oldest type of back-boned animal now living. This type of fish is, indeed, thus proved to have endured throughout the whole of the immense period during which the entire series of secondary and tertiary rocks were deposited ; and when we reflect that the secondary rocks include those enormous accumulations of strata known as the trias, lias, oolites, greensands, and chalk, while the tertiary comprises the threefold divisions termed eocene, miocene, and pliocene, we can scarcely fail to be almost lost in wonder at the prodigious length of time during which baramundas have existed, with but comparatively slight structural modification. The fossil baramundas occur in the secondary rocks of Europe, Africa, India, and North America, and it is an interesting subject of speculation why the group should have totally disappeared from all those regions, to find a last home in far Australia.

The existing baramunda lives mainly or entirely on leaves, and we may therefore conclude that the fossil teeth likewise pertained to fishes which subsisted on somewhat similar nutriment. If, however, we were to infer that all the teeth of fossil fishes which have a ridged or flattened grinding surface belonged to herbivorous types, we should be sadly in error, since many of them, approximating more or less markedly to the *Ceratodus* type, really indicate fishes allied to the well-known Port Jackson shark, in which the mouth is covered with a complete pavement of flattened teeth adapted for crushing shell-fish and other hard animal substances. In all such investigations, the truth can, indeed, only be found out by careful induction, and by availing ourselves of every scrap of information left to us among the relics of former epochs.

RADIANT MATTER.

By A. Jameson.

An article on "Radiometry," that appeared in a recent issue of Knowledge, shortly described how some of the energy of ether waves, striking an absorbent surface, and being transformed into heat—that is, into disturbance of the molecules of matter—may give rise to what is known as Crookes' pressure, and to mechanical motion. It was explained that light mills, as ordinarily made, will only operate when surrounded by a gas in "the fourth condition," *i.e.*, where the free paths of the molecules were long compared with the diameter of the light mill. It is now proposed to relate briefly some of the other experimental facts through which the discovery of this condition of matter has so much improved our knowledge of the actual condition in which gaseous matter exists—that is, of the way in which the molecules of gases are continually moving.

It is perhaps unnecessary to preface the remark that this nominal distinction between gaseous and ultra-gaseous matter is, although a useful, a purely arbitrary one, and that the two conditions are really in the strictest sense of the word continuous ; for the accepted definition of a gas is that it is matter, any portion of which is capable of expanding indefinitely, so as to exert a pressure upon the walls of any containing vessel, however large. Not that even this broad definition is altogether unexceptional. It might be said, for instance, to be inapplicable in the case of those gaseous masses that were described in Mr. Ranyard's article last July, and the extreme limits of which could probably be calculated in the same way and with the same degree of precision as that of the earth's atmosphere was calculated in Knowledge for November last. But this point is only mentioned incidentally ; we are not much concerned with it at present. Gas molecules at a given temperature, or that are moving relatively to one another at a given average speed, are, one might suppose, unaffected individually by the pressure of the gas of which they are constituents. But there are slight exceptions to be taken to this statement also. We have, for instance, the mutual actions (attractions, and perhaps some additional inter-molecular action of adjacent molecules) making themselves felt in proportion to the square of the density, and constituting the well-known deviation from Boyle's law.* Another distinction that will be mentioned is of greater interest in connection with the subject of high exhaustion. It must be premised that molecules themselves are elastic bodies whose constituent parts are capable of oscillatory motions (intra-molecular vibrations). These relative motions of the parts of individual molecules are excited by transverse ether waves, and by molecular impacts. The average velocity of translation of the molecules of a gas represents its sensible heat ; while in addition to this we have the energy concerned in the pulsations of individual molecules or chemical atoms. The intra-molecular vibrations, excited as has been described, are capable in their turn of causing locomotion of the molecule, by virtue of the reaction that takes place when this pulsating body comes in contact with any resisting body. They also give rise to radiations of transverse waves in the ether, and in fact Prevost's law of exchanges of radiant heat holds as perfectly between the molecules of a gas as in any other circumstance. Each molecule is continually throwing off ether waves at the expense of its intra-molecular energy, and is continually deriving reinforcement of that energy from the ethereal radiations of neighbouring molecules, or of surrounding substances. Now in the case, for example, of a gas undergoing the process of cooling, the oscillations of the chemical atoms die away to some extent between successive encounters, owing to the radiation of their energy. Just as a vibrating bell throws off aerial waves, so a vibrating molecule generates waves of a somewhat different character in the medium with which it is surrounded ; and if the material vibrations excited by striking the elastic body are renewed less frequently, the result, similar in each case, is a diminution of the average intensity of those oscillations. Hence very highly rarefied gases, in which the interval of time between successive collisions is considerable,† in which, that is to say, the mean free path is greatly extended, must cool more slowly by radiation than would be the case were they in a condition of greater density. Such distinctions as these between the gaseous and ultra-gaseous states serve, however, to accen-

* Boyle's law, as the reader is doubtless aware, asserts that the pressure and volume of a gas are inversely proportional to one another.

† This interval in hydrogen at the ordinary temperature and pressure is, according to Professor Tait, only one 17,700,000,000th part of a second, that is to say, each particle of hydrogen has the direction of its motion changed on an average 17,700,000,000 times per second.

tuate the fact that for all essential purposes the slight differences between them are simply of degree and not of kind.

We have adopted provisionally, and merely for the sake of simplicity, the view that inter-molecular motions (the relative motions of molecules as wholes) take place in perfectly straight lines. This is certainly a near approximation to the truth. It is therefore reasonable to expect that in gases of ordinary density—in the open air for instance—effects identical with those described under "Radiometry" in the July issue of Knowledge should be obtainable; all that is necessary being that the hot and cold surfaces should be brought sufficiently closely into proximity. This has been accomplished in several ways: notably, by means of an "otheoscope" specially constructed with this object by Prof. Crookes. Another example, familiar to everyone, of molecular pressure exhibited in gas not much removed from the normal density is furnished by the so-called calorific paradox or spheroidal state. When a globule of water is placed on a red-hot stove-plate, evaporation gives rise to such a shower of radiant molecules, raining down on the hot metal from the lower surface of the liquid spheroid, that a mechanical reaction is produced equal to sustaining the weight of the drop, so that this rolls on, without touching, the heated surface. Everyone must have noticed the rapid tremulous agitation of a drop of water undergoing slow evaporation in this way; and it may not be out of place to suggest, for illustrative purposes, that these oscillations exemplify very crudely what is meant by intra-molecular vibration; only, of course, just as a molecule is very much smaller, so its vibrations are very much more rapid than anything within the cognizance of our senses.

Now to return to the consideration of gases so conditioned that the peculiarities of their inter-molecular motions may be most directly and readily shown. The description of only a few of the experiments by which Prof. Crookes has so thoroughly explored these matters, will give extension and confirmation to such explanations as have already been given. One of the first questions that presents itself is, Does *specular* reflection take place at the surface of a body from which a molecule rebounds, and is the angle of rebounding equal to the angle of incidence? The answer to this question is in the negative; for if specular reflection occurred, the only effect that could be produced by the impact of molecules would be a pressure *normal* to the surface struck. The oblique component, if any, of the molecular pressure, acting in the line of incidence, would be exactly neutralized by the oblique reaction in the line of reflection. But it is well known that the oblique component of molecular pressure is not compensated in this way. Fig. 1 shows a form of radiometer by which this fact is very prettily demonstrated. The skewed vanes are, as usual, coated with lampblack, but do not rotate. They are permanently fixed to the upright in the position shown. Above them is a movable disc of thin, smooth mica, supported in the centre upon a needle point. The upper surface of the disc is painted in sectors with pigments representing the different colours of the spectrum duly proportioned; and it is made to rotate with such rapidity by the oblique streams of molecules from the skewed lampblacked vanes, that the colours blend with one another to a neutral grey. Although it is true that some highly polished surfaces are moved by the oblique component of molecular pressure in a way inconsistent with simple reflection, there is of course considerable loss of power in such an application of the molecular pressure to the production of molar motion. Thus with the ordinary form of radiometer one is not surprised to learn that much more energetic effects are obtained when a narrow hoop of metal, transversely stepped or corrugated and painted with lampblack, is placed, for a reacting surface, horizontally within the glass globe. Under this arrangement the energy of the projected molecules is more perfectly absorbed at the moment of impact; and the "cooled," *i.e.* retarded, molecules may even be returned directly back upon the vane or fly. Indeed, a single molecule may thus be bandied very many times in succession between the vane and the reacting surface; and Prof. Crookes has pointed out that with a sufficient degree of rarefaction differences of molecular pressure may exist between different parts of the apparatus for twenty minutes or longer.

Fig. 1.

It has been asserted that the molecular wind from a heated surface blows very nearly normally to the surface; but the fact that radiant matter is also thrown off in oblique directions has been shown by the following experiment. A single "vane" resembling the half of a cone of perhaps one inch in height was made (hollow for the sake of lightness) out of thin sheet mica, and it was fixed on the end of an arm, centrally pivoted, in the same way as a magnetic compass needle. The opposite extremity of the arm carried a small metal counterpoise; and the whole was mounted in a radiometer bulb, exhausted of course, and having within it, below the pivoted arm, a large horizontal disc of lampblacked material. The semicircular base of the mica vane was horizontal, and therefore parallel with the blackened or radiant surface. The triangular (flat) surface of the vane was normal to the blackened disc, and its apex was uppermost. Hence molecules thrown off vertically from the blackened surface would simply exert an upward pressure on the semicircular base of the vane. They could not act in any way upon either the straight or the curved side thereof. Molecules projected obliquely might, on the other hand, impinge on the vertical plane side, or, *but to a less extent*, on the sloping curved side of the vane. For the same degree of obliquity of the molecular showers, the angle of incidence would be great on the vertical side and small on the sloping side of the vane. Hence it is clear, upon the whole, that oblique molecular currents would be deflected to the greatest extent by the vertical plane surface; and that their existence involves the manifestation at this surface of an excess of molecular pressure. As a matter of fact, the pressure on the plain side of the vane is enough to set the arm in rotation.

Radiometers have been made in which one of the horizontal arms to which the vanes are attached is a magnetic needle. Of course, the rotation of the vanes in such an instrument can be stopped by simply bringing a powerful bar magnet into the neighbourhood of the bulb. If this is done while the instrument floats, like a hydrometer, in a glass jar filled with water, the showers of molecules from the stationary vanes will set the glass envelope itself into rotation. A paper index, of say one foot in length, may be attached to the upper surface of the glass globe and will make this revolution more evident. Such an arrangement, in which the motion produced by light is not only visible but tangible, makes an exceedingly attractive scientific toy. Prof. Crookes suggests also that the *rotation* of the magnetic needle within such an instrument as this might be made to induce electric currents in wires placed outside the bulb, thereby operating self-

recording devices that would be most useful in meteorological work.

The efficiency of the light in different parts of the solar spectrum for producing motion at the lampblacked surface in a radiometer has been found to be as follows: Assuming that of the ultra red waves to be represented by 100 the extreme red gives 85 per cent. of this standard repulsion; red, 73; orange, 66; yellow, 57; green, 41; blue, 22: indigo, $8\frac{1}{2}$; violet, 6; and ultra violet 5 per cent.

It has been suggested that a kind of mechanical process of photography might be based upon the different repulsive powers of different kinds of light. A still more interesting adaptation of the phenomena of radiant matter to purposes connected with photography, and one that has been practically carried out by Prof. Crookes, is in the study of those thermal changes which accompany photographic action at a surface. Suppose a miniature Daguerreotype plate delicately suspended in a "radiant matter bulb," the exhaustion and sealing of the bulb having been carried out in non-actinic light. If, when the apparatus is at rest and in thermal equilibrium in a dark room, a beam of actinic light is directed upon the sensitive surface, of course a certain amount of repulsion, due to molecular pressure, will take place; but such repulsion is a result of the conversion of light into heat. And hence, for so long as a great part of the incident radiation is engaged in performing the work of chemical separation, the repulsion due to that radiation must be considerably below the normal. Space will not permit, or rather it would be inappropriate to the subject of these articles, to introduce a discussion of the results of these experiments, and of others equally interesting, bearing on the absorption of the different coloured powders used as coating upon radiometric vanes. Of course, the wave-length of the radiant heat or light that is employed is an important factor that has to be kept in view in the interpretation of all such experiments.

When radiation of very low frequency is employed, it may be so largely absorbed by the glass bulb that this, in consequence of the increase of its temperature, becomes itself a generating surface for molecular wind. Effects of this kind, which may be produced also by placing a warm finger on the bulb, give rise to movements of the vanes that are at first sight very puzzling. Again, a thin platinum wire, mounted within the exhausted globe (see Fig. 2), may be so heated by being made part of a galvanic circuit as to generate very powerful currents of radiant matter, and produce mechanical effects of proportionate intensity on a "turbine wheel" radiometer. The skewed vanes in this little heat engine are usually made of sheet mica; but, of course, the nature of the material is unimportant, so long as rigid and light, for the turbine wheel simply plays the same part in relation to radiant matter that the paddles of a water-wheel, or the sails of a windmill, do to currents of water or air.

Fig. 2.

THE FACE OF THE SKY FOR SEPTEMBER.

By Herbert Sadler, F.R.A.S.

The solar disc shows little or no diminution in the frequency of groups of spots and faculæ. The following are conveniently observable minima of Algol: September 1st, 9h. 49m. p.m.; September 4th, 6h. 38m. p.m.; September 24th, 8h. 20m. p.m.

Mercury is a morning star throughout September, and is well situated for observation during the second and third weeks of the month. On the 8th he rises at 3h. 49m. a.m., or 1h. 36m. before the Sun, with a northern declination of $11\frac{1}{2}°$ and an apparent diameter of $7\frac{1}{2}''$, about $\frac{34}{100}$ths of the disc being illuminated. On the 13th he rises at 3h. 51m. a.m., or 1h. 42m. before the Sun, with a northern declination of 10° 46′ and an apparent diameter of $6\frac{1}{3}''$, about $\frac{55}{100}$ths of the disc being illuminated. On the 16th he rises at 4h. 0m. a.m., or 1h. 39m. before the Sun, with a northern declination of 9° 36′ and an apparent diameter of 6·0″, $\frac{7}{10}$ths of the disc being illuminated. On the 23rd he rises at 4h. 38m. a.m., or 1h. 12m. before the Sun, with a northern declination of 5° 22′ and an apparent diameter of $5\frac{1}{2}''$, about $\frac{9}{10}$ths of the disc being illuminated. After this he approaches the Sun too closely to be conveniently observed. He is at his greatest western elongation (18°) on the 11th, and his greatest brightness about the 17th. While visible, Mercury describes a direct path through Leo, being near Regulus on the 7th.

Venus is still a very conspicuous object in the morning sky, but both her brightness and diameter decrease considerably during the month. She rises on the 1st at 1h. 24m. a.m.. with a northern declination of 17° 23′ and an apparent diameter of $28\frac{3}{4}''$, $\frac{40}{100}$ths of her disc being then illuminated, and her brightness being about the same as in the middle of May. On the 30th she rises at 1h. 44m. a.m., with a northern declination of 13° 6′ and an apparent diameter of 21″, $\frac{57}{100}$ths of the disc being illuminated, and her brightness having diminished to what it was at the beginning of April. She is at her greatest western elongation (46°) on the 19th. At 3h. 50m. a.m. on the 22nd a $7\frac{1}{2}$ magnitude star will be 1′ south of the planet. During the month Venus describes a direct path from the boundaries of Gemini through Cancer into Leo.

Mars is an evening star, but is still wretchedly situated for observation in these latitudes. On the 1st he rises at 6h. 10m. p.m. with a southern declination of $21\frac{1}{4}°$ and an apparent diameter of $22\frac{1}{4}''$, the defect of illumination on the following limb becoming now very perceptible. On the 30th the planet sets at 0h. 33m. a.m., with a southern declination of 21° 11′ and an apparent diameter of 17·0″, his brightness towards the end of the month being less than half of what it was at opposition. During the month he describes a direct path in Capricornus. The minor planet Pallas will be in opposition on September 20th, on which evening she souths at midnight with a southern declination of 6° 8′. The present opposition is not a very favourable one, the stellar magnitude of the planet being only $7\frac{1}{2}$. During the month Pallas describes a retrograde path in Cetus.

Jupiter is now becoming a magnificent object in the evening sky, being actually visible to the naked eye in sunlight at the end of the month, the coming opposition being a very favourable one. He rises on the 1st at 8h. 1m. p.m., with a northern declination of 8° 1′ and an apparent equatorial diameter of $46\frac{1}{2}''$. On the 30th he rises at 6h. 3m. p.m., with a northern declination of 6° 54′ and an apparent equatorial diameter of 49″. During the month he describes a retrograde path in Pisces. The following phenomena of the satellites occur before midnight, while Jupiter is more than 8° above and the Sun 8° below the horizon. A transit ingress of the second satellite at 9h. 49m. p.m., and a transit egress of its shadow at 10h. 24m. p.m. on the 1st. An eclipse disappearance of the first satellite at 10h. 5m. 26s. on the 2nd. A transit egress of the shadow of the first satellite at 9h. 36m. p.m., and of the satellite itself at 10h. 30m. p.m. on the 3rd. An eclipse reappearance of the third satellite at 9h. 49m. 48s., and an occultation dis-

appearance of the same satellite at 11h. 23m. p.m. on the 6th. A transit ingress of the shadow of the second satellite at 10h. 27m. p.m. on the 8th. An eclipse disappearance of the first satellite at midnight on the 9th. A transit ingress of the shadow of the first satellite at 9h. 16m. p.m.; an occultation reappearance of the second satellite at 9h. 25m. p.m.; a transit ingress of the first satellite at 10h. 4m. p.m.; and a transit egress of the shadow of the same satellite at 11h. 30m. p.m. on the 10th. An occultation reappearance of the first satellite at 9h. 26m. p.m. on the 11th. An eclipse disappearance of the third satellite at 11h. 38m. 13s. p.m. on the 13th. An eclipse disappearance of the second satellite at 7h. 59m. 53s. p.m.; a transit ingress of the shadow of the first satellite at 11h. 10m. p.m.; an occultation reappearance of the second satellite at 11h. 41m. p.m.; and a transit ingress of the first satellite at 11h. 49m. p.m. on the 17th. An eclipse disappearance of the first satellite at 8h. 23m. 20s. on the 18th. A transit egress of the first satellite at 8h. 27m. p.m. on the 19th. A transit ingress of the third satellite at 7h. 58m.; a transit egress of the shadow at 8h. 8m. p.m.; a transit egress of the satellite at 9h. 42m. p.m.; and an eclipse disappearance of the second satellite at 10h. 35m. 1s. on the 24th. This transit should be carefully watched. An eclipse disappearance of the first satellite at 10h. 18m. 11s. on the 25th. A transit ingress of the first satellite at 7h. 33m.; a transit egress of the shadow of the second satellite at 7h. 35m. p.m.; a transit ingress of the first satellite at 7h. 59m. p.m.; a transit egress of the second satellite at 8h. 20m. p.m.; a transit egress of the shadow of the first satellite at 9h. 46m. p.m.; and a transit egress of the shadow itself at 10h. 10m. p.m. on the 26th. An occultation reappearance of the first satellite at 7h. 21m. p.m. on the 27th.

Both Saturn and Uranus are invisible, and as Neptune does not come into opposition till December we defer an ephemeris of him till next month.

There are no very well marked showers of shooting stars in September.

The Moon is full at 9h. 7½m. p.m. on the 6th; enters her last quarter at 0h. 50m. p.m. on the 13th; is new at 1h. 16m. a.m. on the 21st; and enters her first quarter at 6h. 19m. a.m. on the 29th. She is in perigree at 11·0h. p.m. on the 8th (distance from the earth 225,550 miles), and is in apogee at 6·0h. p.m. on the 24th (distance from the earth 252,140 miles). The greatest western libration takes place at 9h. 15m. a.m. on the 3rd, and the greatest eastern at 5h. 10m. a.m. on the 16th.

Chess Column.

By C. D. Locock, B.A.Oxon.

All communications for this column should be addressed to the "Chess Editor, *Knowledge Office*," and posted before the 10th of each month.

Solution of August Problem (by D. R.)

1. R to R6, and mates next move.

Correct Solutions received from Alpha, H. S. Brandreth, W. Pennett, G. Burt, and A. R.

G. Burt.—Some of your variations, however, are incorrect. There are four different mates for the moves of the Black Bishop. (The Queen cannot mate.) After 1. . . . P to Q3, 2. Kt to B4 is the mate.

Alpha.—Generally speaking, the more variations the easier the problem; for some of the variations have to be ready-made. Duals result from Kt to Rsq or R5.

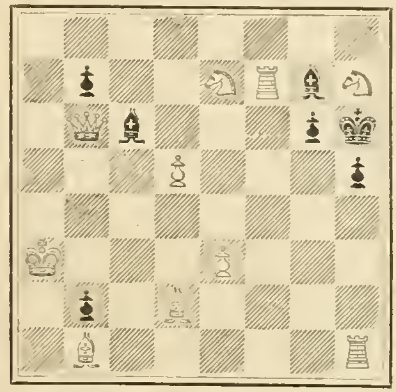
G. K. Ansell.—Thanks for the problem, which has been returned to you for revision.

PROBLEM.

By C. D. P. Hamilton.

(2nd prize in *Illustrated American* Tourney.)

Black.



White.

White to play, and mate in two moves.

The following fine game was played in the fourth round of the Dresden Tournament.

"Ruy Lopez."

White (Mason).	Black (Scheve).
1. P to K4	1. P to K4
2. Kt to KB3	2. Kt to KQ3
3. B to Kt5	3. P to QR3
4. B to R4	4. Kt to B3
5. P to Q3	5. P to Q3
6. P to B3	6. B to K2 (*a*)
7. QKt to Q2	7. Castles
8. Kt to Bsq	8. P to QKt4
9. B to B2 !	9. P to Q4
10. Q to K2 (*b*)	10. R to Ksq
11. Kt to Kt3	11. P to Q5 (*c*)
12. Castles	12. P to R3
13. Kt to Ksq	13. P to Kt5
14. B to R4 (*d*)	14. B to Q2
15. P to QB4	15. B to KBsq
16. P to B4	16. Kt to Ktsq (*e*)
17. P takes P !	17. R takes P (*f*)
18. B takes B (*g*)	18. QKt takes B
19. Kt to B3	19. R to Ksq
20. K to Rsq (*h*)	20. Kt to B4
21. Kt to B5	21. Kt to K3
22. B to Q2	22. P to QR4
23. R to KKtsq !	23. P to R5 (*i*)
24. P to KKt4	24. R to R4 (*j*)
25. QR to KBsq (*k*)	25. P to R6
26. P to Kt3	26. K to R2
27. P to Kt5	27. R takes Kt (*l*)
28. P takes R	28. Kt to B5
29. Q to Qsq	29. KKt to R4
30. B takes Kt (*m*)	30. Kt takes B
31. R to Kt4	31. B to Q3 (*n*)
32. P takes P	32. P takes P
33. P to B5 (*o*)	33. Kt to Q4 !
34. P takes B	34. Kt to K6
35. P takes P !	35. Q to Rsq (*p*)

36. Q to Bsq	36. Kt takes KR
37. Q to KB4	37. Kt to K6
38. R to KKtsq	38. Q to B3 (*q*)
39. P to B6 !	39. Kt to Kt5 (*r*)
40. R takes Kt	40. Resigns.

Notes.

(*a*) A very questionable defence, adopted by Lasker in one of his games with Blackburne. After castling the Bishop becomes a fixture, hampering the movements of Queen and Queen's Knight. P to KKt3 is the correct defence.

(*b*) Preventing the exchange of Queens: *vide* Steinitz *v.* Tschigorin.

(*c*) Here, as usual, a weak move, for the Pawns are liable to ultimate disintegration by P to KB4. Perhaps his best course was 11. . . P×P, 12. P×P, B to Q3. His next move is probably made with the intention of answering 13. Kt to Ksq by 13. . . P to KKt4, a plan from which he imprudently diverges.

(*d*) This excellent move prevents P to KKt4, for White would get in with the Knight to B5, gaining time by attacking a Pawn.

(*e*) 16. . . P×P was certainly a lesser evil. He could then play the Knight to K2.

(*f*) Obviously 17. . . . B×B; 18. P×Kt, P×P; 19. Kt to B5 would be fatal to Black.

(*g*) Best, for it secures possession of KB5 for his Knight.

(*h*) 20. Kt×P is wisely rejected. Black recovers the Pawn by B to B4 followed by Kt to K4, or even simply by Kt to K4. The King's move accordingly is not much use. Perhaps he feared 20. Kt to B5, B to B4, threatening Kt×P: but that could be guarded against when the necessity arises. Black now gains time for ingeniously defending the Pawn twice by bringing his Knight to a better square.

(*i*) In view of the coming attack it may seem better to play K to R2 at once, but the King would be strangely but promptly sent back by 24. Kt to K5! Better to have played P to B4 last move, and followed it by K to R2 and P to Kt3; for Black could have then answered Kt to K5 by Q to B2.

(*j*) Preventing Kt to K5 as in the last note, and preparing to sacrifice the exchange if necessary. See also note (*k*). The defence is most ingenious, and the game now becomes full of surprises.

(*k*) To defend the advanced Knight: for otherwise he cannot play P to Kt5 on account of ultimately R×Kt. It seems doubtful whether it would not be better to move the Queen to B2, attacking a Pawn at the same time, even if it is not quite safe to take it.

(*l*) A highly ingenious resource, which makes the game very difficult for both sides.

(*m*) Over-estimating his advantage. This exchange merely brings the other Knight (which has no move) into play, and loses the command of the point at K3. R to Kt4 looks good.

(*n*) A very clever trap, into which Mr. Mason unnecessarily, though probably intentionally, falls.

(*o*) 33. Kt×P is probably quite as good.

(*p*) Best. If 35. . . . Q×P, 36. Q to Ksq. wins.

(*q*) A mistake, which at once loses a hard-fought game. R to Ktsq was essential (not 38. . . . P to B3, 39. Q to Kt3 and wins). Even after 38. . . . R to Ktsq, 39. P to B6 must win shortly.

(*r*) There is nothing to be done. If 39. . . . R to KKtsq, 40. R×R, K×R; 41. Q to Kt3ch, K to Bsq; 42. Q to Kt7ch, K to Ksq; 43. Q to Kt8ch, K to Q2; 44. Q to Q8ch, etc.

CHESS INTELLIGENCE.

The International Tournament at Dresden resulted as follows:—

1st Prize	Dr. Tarrasch (Nuremberg).
2nd, 3rd Prizes	Makovetz (Austria). Porges (Hungary).
4th, 5th Prizes	Marco (Vienna). Walbrodt (Berlin).
6th, 7th Prizes	von Bardeleben (Berlin). S. Winawer (Warsaw).

The failure of the English contingent is most noticeable, though Mr. Blackburne secured the special prize given for the best score made against the prize winners. Mr. Mason was also for once unplaced, while Mr. Loman was absolutely last. This is Dr. Tarrasch's third consecutive victory in International Tournaments, in which, out of 52 games, he has lost only one—this was to Albin at Dresden, the result of inferior play in the opening.

The winners of the other chief prizes are comparatively unknown in England. Makovetz is perhaps the most talented of the four, Walbrodt being the most difficult to beat. The veteran Winawer made a fine start, but failed to sustain his form.

The Brighton meeting of the Counties Chess Association resulted, as last year, in a victory for Mr. J. H. Blake, of Southampton, who scored 6½ out of a possible 8. Mr. W. V. Wilson, of Brighton, was a good second with a score of 6. Mr. H. W. Butler, also of Brighton, was third; Messrs. Thorold and Trenchard dividing the fourth prize. The remaining players in order were E. Lambert (Exeter), A. Guest (London), A. Rumboll (Bath), and the Rev. A. B. Skipworth, who failed to score. Mr. Skipworth nearly won last year, and must have been handicapped by his duties as Hon. Sec.

The seventh volume of Mr. Morgan's Chess Library consists of a complete collection of all the 46 games played in matches and tournaments between Steinitz and Tschigorin. There are numerous diagrams, and a useful index of the openings, but no notes. The two telegraphic games are included. The price is one shilling.

Contents of No. 82.

TERMS OF SUBSCRIPTION.

"Knowledge" as a Monthly Magazine cannot be registered as a Newspaper for transmission abroad. The Terms of Subscription per annum are therefore as follows:—To any address in the United Kingdom, the Continent, Canada, United States, Egypt, India, and other places in the Postal Union, the Subscription is 6 shillings, including postage; or 1 dollar 50 cents; or 6 marks; or 7 francs 50 cents.

For all places outside the Postal Union, 5 shillings in addition to the postage.

Communications for the Editor and Books for Review should be addressed Editor, "Knowledge" Office, 326, High Holborn, W.C.

KNOWLEDGE

AN ILLUSTRATED

MAGAZINE OF SCIENCE

SIMPLY WORDED—EXACTLY DESCRIBED

LONDON: OCTOBER 1, 1892.

CONTENTS.

BEE PARASITES.—IV.

By E. A. Butler.

(*Continued from page* 167.)

To conclude our papers on the parasites to which British wild bees are subject, it yet remains to consider the most extraordinary of them all, the extremely aberrant beetles belonging to the family *Stylopidæ*. It will be best to introduce these remarkable insects to our readers as they were in the first instance introduced to the British scientific world, and in fact, one may almost say, to the scientific world at large. In the year 1802 the Rev. W. Kirby, Rector of Barham in Suffolk, well known as the joint author with Spence of the "Introduction to Entomology," published an important monograph on British bees, a group of insects to which he had devoted much study, and which he was the first to properly classify and describe. It was while collecting materials for this work that he came across the first *Stylops* ever recorded to have been seen by any English naturalist, and in the above monograph he inserted a description of his discovery in a passage which has since become classical to the student of the *Stylopidæ*. With the acuteness of observation which distinguished him, he had noticed minute protuberances on the bodies of certain bees. These, he thought, were possibly mites of some kind, as he well knew that such creatures are often found on bees' bodies; but whatever they might be, the mere fact that there was something of a mystery was quite sufficient to impel him to inquire into the matter and endeavour to get at the truth of it. He was thus led to his great discovery, but what follows had better be told in his own words. In reference to his examination of the first of these little knobs, he says, "What was my astonishment when, upon attempting to disengage it with a pin, I drew forth from the body of the bee a white fleshy larva, a quarter of an inch long, the head of which I had mistaken for an Acarus." This was half the discovery, but we shall see presently that Kirby was mistaken in supposing that the maggot-like creature he had extracted from the bee's body was a larva; it was, on the contrary, a perfect insect, being in fact the female *Stylops*. For many years, however, it was imagined, and very naturally, considering its form, to be a larva; but further facts as they were discovered were seen to be more and more difficult to reconcile with such a supposition, and at last it was recognized that the footless grub-like creature was the last stage in an eventful history, a mature insect of an extremely degraded type.

But, as we have said, this maggot-like grub was only half the discovery, and that the less romantic half. Mr. Kirby continues: "After I had examined one specimen I attempted to extract a second, and the reader may imagine how greatly my astonishment was increased when, after I had drawn it out but a little way, I saw its skin burst and a head as black as ink, with large staring eyes and antennæ consisting of two branches, break forth and move itself briskly from side to side. It looked like a little imp of darkness just emerging from the infernal regions. I was impatient to become better acquainted with so singular a creature. When it was completely disengaged, and I had secured it from making its escape, I set myself to examine it as carefully as possible, and I found, after a careful inquiry, that I had not only got a nondescript, but also an insect of a new genus, whose very class [*i.e.* order] seemed dubious." This was the other member of the *Stylops* family, the active little male fully equipped with wings and legs, and as great a contrast to his mate as could possibly be imagined. Mr. Kirby had been extremely fortunate in securing both sexes on the same occasion, for the males are far less numerous than the females; F. Smith estimated that the usual proportion was about one male to twenty females. Of course, Kirby did not recognize the two creatures as thus related to one another, but looked upon one as the larva and the other as the perfect insect, never dreaming that there would be such discrepancy in the forms of the adult insect. As he did not know anything of the life-history of his nondescript, he failed to see any close resemblance between it and any of the types of the usually recognized orders of insects: he therefore proposed to put it in an order by itself, or at least accompanied by another very similar insect, which had just been discovered by a continental observer in the body of a wasp, and had been thought by him to be a kind of ichneumon fly. This new order was called Strepsiptera (twisted wings) in allusion to the curiously bent or twisted form of the rudimentary fore wings. When, however, its life-history came to be unravelled, it was seen that there were strong resemblances between *Stylops* and *Meloë*, so that the most natural place for its family was evidently amongst the Coleoptera, near to the oil beetles. There it and its few relatives which have been thus far discovered are therefore at present resting.

Let us now look a little more closely at our two insects, and then we may enquire how they came to be imbedded in the bee's body in which they were found. And first as to the male (Fig. 9). There are several species of

Stylopidæ, but they are all very minute creatures, the largest being not more than a quarter of an inch long, while the rest are considerably smaller. The species in question has a jet black body and a pair of large milk-white wings. Its eyes are large and staring, appearing almost as if perched on the end of stalks, but the facets of which they are composed are very few, not more than about fifteen to each eye; the antennæ, most unusually, are each composed of *two* branches side by side. The fore wings are reduced to extremely minute dimensions, and bent as above described; what their use can be is quite conjectural. The hind wings, on the other hand, are ample, each spreading, when fully extended, over about a quadrant of a circle. Connected with this great development of hind wings is the large size of the hinder portion of the thorax, to which the wings are attached, and which, therefore, lodges the muscles by which they are rapidly vibrated. When not in use, the wings are folded longitudinally and laid along the back. The body is small and weak and, when the insect dies, soon shrivels up. F. Smith says, "The texture of all parts of the body of a male *Stylops* is of so delicate a nature that within two hours after death the entire appearance of the insect is changed, bearing no nearer a resemblance to the living creature than a shrivelled mummy does to the once manly and graceful Egyptian."

Fig. 9.—Male *Stylops*. (After Westwood.)

The little creature lives apparently but a few hours, and during that time takes no food, though it has a pair of jaws, being intent upon its sole business, the discovery of a mate. But in the pursuit of this business it manages to squeeze a good deal of activity and movement into its brief existence. As may be imagined, it is a by no means frequent occurrence to see these ephemeral beings on the wing, and only a few such opportunities have been recorded. Dale describes a *Stylops* as flying "with an undulating or vacillating motion amongst the young shoots of a quickset hedge," and adds, "I could not catch it until it settled upon one, when it ran up and down, its wings in motion, and making a considerable buzz or hum. . . . . I put it under a glass and placed it in the sun; it became quite furious in its confinement, and never ceased moving about for two hours. . . . . It buzzed against the side of the glass with its head touching it, and tumbling about on its back." Thwaites, again, speaks of them as being "exceedingly graceful in their flight, taking long sweeps as if carried along by a gentle breeze," usually flying so high as to be out of reach, but "occasionally descending and hovering a few inches from the ground. . . . . When captured they are exceedingly active, running up and down the sides of the bottle in which they are confined, moving their wings and antennæ very rapidly."

The search for a partner cannot be a very easy matter, seeing that the destined bride is completely imbedded in the body of a bee, and no part of her is visible outside except the minute knob-like fore-part, and this must be quite unnoticeable while the bee is on the wing. Hence, this matrimonial hunt must be an exciting occupation, especially as the hours pass by, and the sands of the hunter's little life are running rapidly down. No wonder, therefore, that its senses are preternaturally quickened, as we conclude to be the case from the fantastically complicated antennæ, and the prominent eyes, so placed as to take in the whole horizon at a sweep.

The female is shaped something like a phial bottle, the narrow part being connected with the broader by a still narrower constriction. The small anterior part consists of the head and thorax fused into one, and it is a portion of this that projects between the segments of the bee's back. The rest of the creature lies buried in the bee's abdomen in the manner shown in Fig. 10, occupying, sometimes, as much as one-fifth of the whole space. As regards organization, the condition of this curious being might be not inaptly expressed in the words with which Shakespeare closes his description of the seven ages of man:

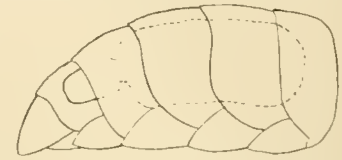


Fig. 10.—Outline of female *Stylops* in abdomen of Bee. (After Paskard.)

"Sans teeth, sans eyes, sans taste, *sans everything*."

She has neither legs nor wings, and never leaves the body of her host. Such food as she takes appears to enter her body by absorption from the surrounding tissues of the bee, and her abdomen becomes simply a chamber for the development of the eggs and the hatching of the young. She produces an immense number of larvæ viviparously, which, as soon as hatched, escape through an aperture in her thorax, which is the natural orifice of the reproductive organs transposed from its usual position to the only spot in which it would be of any use. The hatching of the larvæ is followed by the death of their parent. The larvæ are extremely minute, being not more than $\frac{1}{40}$th of an inch in length; they swarm in hundreds over the body of the bee, clinging to its hairs, and giving it the appearance of being covered with dust. From Fig. 11 it will be seen that the larva in its first condition bears not the slightest resemblance to either of its parents, being an active six-legged creature with long hair-like appendages at the tail, just as is the case with the newly-hatched larva of the oil beetle. It has, however, no claws at the end of its feet. The bee seems to object to the presence of the larvæ on its body, and their emission apparently causes it considerable annoyance and irritation, and renders it very excited. A stylopized female of *Andrena Trimmerana* was once kept for some days by F. Smith, enclosed in a gauze-covered box, and continually kept supplied with fresh flowers. One day he noticed the bee "running about apparently in a very excited state, burying herself beneath the leaves and flowers, then issuing forth and running round the sides of the box; sometimes she would stop, bury her head in the petals of a dandelion, and then commence brushing herself with her posterior legs, passing them quickly over the upper surface of the abdomen." Closer examination showed that she was covered with *Stylops* larvæ, which she was doing her best to get rid of. No doubt large numbers of the larvæ perish in this way, being brushed off and falling into situations in which they get no chance of mounting on bee-back again.

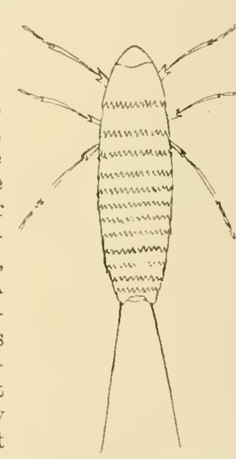


Fig. 11.—Larva of *Stylops*. (After Newport.)

Like the larvæ of *Meloë*, it is now essential that the young *Stylops* should be conveyed to the cell of a bee, since

its next move must be to enter the body of a bee larva, which will thus be selected as its lifelong host. Now here arises a very nice question; in order to take this step in its advancement, will it avail the *Stylops* larva to remain on the body of the bee that produced it, or is it necessary that it should leave its foster parent and seek out an unstylopized specimen? This evidently turns again on the question whether the stylopized bee is capable of reproducing its kind, a question somewhat more complex than might at first be imagined, and in our endeavours to answer it we must consider for a while what effects the parasitism of the *Stylops* produces on the bee. These are very considerable, and may be classified into two groups, internal and external effects. The internal effects run chiefly in the direction of the reduction in size of the various organs of the bee's abdomen. It is obvious that the presence of so large a foreign body amongst the bee's vitals must produce pressure upon surrounding parts and interfere with their proper growth and development. Thus the digestive tract of the bee is lessened in size and its walls become thinner, whereby the fluids it contains more easily pass through into the body of the parasite. But the effect is most marked in the case of the reproductive organs, which are much reduced in size and retarded in development. Both sexes of the bees are liable to the attack of the parasite, though the females, as a rule, seem to be the more frequently selected. The exact amount of interference with the bee's well-being which the parasite produces varies a good deal, according to the species of bee, the sex of both parasite and host, and the number of parasites present, which may be as much as four or five in one specimen, though most frequently they are solitary. Dissection of the bee's body has, therefore, led to somewhat conflicting results, some observers chronicling the almost entire abortion of the reproductive organs and the entire absence of their characteristic products, and others noting only their reduction in size and retardation in development.

But even supposing the female stylopized bee to be able to produce fertile eggs, this is by no means all that is necessary to ensure the production of a brood. The young bee grub must be provided with food, and to this end pollen must be collected, and this cannot be done in the absence of the pollen-collecting organs. This leads us to consider the external changes produced by stylopization. The exact details here again will differ in different cases. Our present business is with the female bee, and we will therefore consider only the changes produced in the external appearance of a female bee of the genus *Andrena*. These have been very clearly pointed out by Prof. Perez in a communication made to the Linnæan Society of Bordeaux, and his observations have been confirmed by other observers. The head of the bee becomes smaller and the abdomen more globose, while the hairs tend to become less densely distributed and paler in tint, and to form bands at the edges of the segments. The two chief parts of the pollen-collecting apparatus in an *Andrena* consist of a brush of hairs on the outside of the posterior tibiæ, and a little curl or lock of rather long hairs on the posterior trochanters, the minute joint which is situated between the basal joint which attaches the leg to the body and the thigh. Now in the stylopized bee these masses of hair become shorter, and therefore less fitted for the collection of pollen. In fact, in most respects the female bee approaches the condition of the male, while the stylopized male, on the other hand, tends to assume the characters of the female. M. Perez, who has examined great numbers of these bees, says that he has only met with *one* single specimen of a female stylopized *Andrena* which was carrying pollen on its collecting brushes. Now if it be the case, as is generally believed, that a bee does not collect pollen till it is fertilized, this would seem to indicate that stylopized bees do not become fertilized. If this be so, then, evidently, the only chance that the newly-hatched larvæ, which we left scrambling about amongst the forest of hairs on the body of their foster parent, have of further development is to dismount their present steed and seek another which is not thus handicapped; and it is clear that such a step will, under any circumstances, be absolutely necessary in the case of those produced from *male* bees. This is evidently, then, a dangerous and critical period in the little creature's career, and amid its many risks no doubt vast numbers perish, just as do the *Meloë* larvæ. That such must be the case is indeed evident from the contrast between the multitudes of larvæ produced and the comparative scarcity of the perfect insect.

Suppose, however, that a larva has successfully gained the back of an unstylopized female bee, and is by her conveyed to the cell she has made and provisioned for her own larva. The *Stylops* larva now dismounts, and in some way insinuates itself into the body of the bee grub, subsequently casting its skin and losing its legs, becoming itself maggot-like, and thus adapted to the new phase of life upon which it has entered. In this larva it remains, growing with its growth, and passing through its own metamorphoses as its host does, so that it becomes matured at the same time that the bee puts on its final form. During its preliminary stages it lies with its head towards the head of the bee, the position in which it made its entrance into the bee's body, but previously to maturing it reverses its position, and thus is enabled, if a male, to make its exit into the outside world head first, just as Kirby witnessed it, or if a female, is placed in a suitable position for fulfilling the functions that yet remain for her before she can give birth to her enormously multitudinous family. Having thus followed the course of the development of the *Stylops* throughout its life-history, we see that it does not, as so many internal parasites do, cause the death of the individual host, though, if the bees are really rendered abortive by its presence, it tends to the extinction of the *species*.

Stylopization, in some cases, so greatly alters the bee's appearance as to have led to the erroneous creation of pairs of species, the stylopized form having been described and named as a distinct species from the bee in its natural condition. Besides the sexual reversals already enumerated, there is one very curious result of stylopization for which it is difficult to find a physiological explanation; this is the alteration that takes place in the colour of the face in some bees. There are certain bees of the genus *Andrena*, notably *A. labialis*, in which the face of the male is yellowish white, while that of the female is blackish. Stylopized specimens of such species, if they are males, tend to have the pale parts reduced in size and altered in shape, while the females show a tendency to appear with spots of yellow on the usually dark face.

Erratum.—In Mr. E. A. Butler's article, "Bee Parasites," on page 164 of our last number, the inscription under Fig. 8 should read, "Stages of larva of *Sitaris muralis*. (After Fabre)."

THE FUEL OF THE BLAST FURNACE.

By Vaughan Cornish, M.Sc., F.C.S.

In a former article (Knowledge, February, 1892) we traced the progress of our knowledge of the element carbon, from the time when it was recognized as a reducing principle by means of which ores could be made to yield the metals, to the time when Lavoisier

showed that in this process of reduction the element carbon abstracts the oxygen from the ore, forming carbonic acid. It was pointed out, also, that in such *reducing* processes the carbon plays a two-fold part, for while one portion unites with the oxygen of the air, and in this process generates heat, the high temperature thus brought about enables the remainder of the carbon to abstract the oxygen from the ore, forming more carbonic acid, and leaving the metal. It is merely a matter of arrangement and of disposition of the parts of apparatus whether one portion of carbon is used as the heating, and a separate portion as the reducing agent, or whether the two processes are carried on simultaneously in the same parcel of the material. The former condition obtains, for example, when a powdered metallic oxide mixed with finely-divided charcoal is placed in a glass or porcelain tube, which is heated in a charcoal furnace. The second condition would obtain when the common hæmatite iron ore (an oxide) was mixed with charcoal in the old-fashioned iron furnace of Spain, and the charcoal having been set alight, and the heat having been urged by a blast of air, the iron was reduced to the metallic state by the action of the carbon.

In any process of iron smelting in which an oxide of iron is reduced by some form of carbon the initial and the final states may be represented thus :—

Oxide of iron + carbon = metallic iron + carbonic acid.

The symbol = must here be interpreted "may be caused to yield." But as our acquaintance with a chemical process becomes more intimate we generally find that a knowledge of the initial and final states of matter is not sufficient either for a proper understanding of what goes on, or, in many cases of manufacturing processes, for regulating the process so as to obtain a good "yield" of the valuable product. For this it is often necessary to learn the steps between the initial and final stages, even if the "intermediate" bodies formed have but a transitory existence. This is especially the case in the reduction of iron ore in the blast furnace. The modern improvements which enable manufacturers to keep pace with the demand for iron roads and iron ships are based upon the conditions of formation and decomposition of the lower oxide of carbon, carbon monoxide gas, which is formed either in the incomplete combustion of carbon, or when heated carbon acts upon carbonic acid. Carbon monoxide gas readily takes fire in presence of oxygen, burning with formation of carbonic acid. In so doing the carbon takes up a second dose of oxygen, being combined in carbonic acid with just twice as much oxygen as in the lower oxide. Carbon monoxide also acts as a reducing agent, the gas at a high temperature extracting the oxygen from metallic ores.

If, in any metallurgical process a portion of the coal, coke, or charcoal used is only burnt to carbon monoxide, then, in respect of that portion, we only get a part of the heating effect of the fuel, and half the reducing effect of the reducing agent. True, the hot carbon monoxide issuing from the apparatus burns in the air, forming the final product, carbonic acid, and producing more heat; but we want that heating effect in the apparatus, not outside it, and we wish, if it be possible, that the extra dose of oxygen required for complete combustion of the monoxide should come from the ore and not from the atmosphere.

In the blast furnace the materials put in at the top are oxide of iron, coke (*i.e.*, *charred* coal), and limestone. The lime of the limestone combining with siliceous matter mixed with the ore removes these impurities in the form of a light fusible slag, which floats upon the top of the liquid iron collected at the bottom of the furnace. The air is supplied in a forced blast through tubes called twyeres, placed near the bottom of the furnace. The furnace is always kept full, or nearly full, and we may consider the conditions at any part of the furnace as remaining constant, as, when once lighted, the furnace is kept always in operation till it is "blown out," after perhaps twelve or fourteen years' work. The furnaces used at the beginning of the century were about forty-eight feet high. Those used now are eighty feet. It was found that by giving more time to the ascending current of carbon monoxide a greater proportion of ore was reduced. A great deal of the gas in the smaller furnace escaped without, so to speak, having the chance of becoming fully oxidized.

A still more important improvement was the introduction of the *hot blast*. It appears paradoxical that one ton of coke burnt to heat the air before entering the furnace should serve three tons of coke in the furnace itself; yet so it is. The heating (with hot blast) being partly done from outside, it is not necessary to put so much coke into the furnace; consequently, there is room for more of the ore. The power of the ore and limestone to intercept heat is double that of the coke which has been replaced, and there is a greater surface of the ore to act on the diminished and slower current of carbon monoxide. Hence, greater economy in the production of pig-iron was effected by the use of the hot blast, and by raising the height of the furnace to eighty feet. Some furnaces were built of one hundred and three feet, but it was found that no further economy was effected. The present approved dimensions of furnace and temperature of blast appear to give a better yield of metal than if the furnace be larger and the blast hotter, and a better yield than if the furnace be smaller and the blast colder. The reason is as follows:—We have together in the furnace, carbon monoxide and iron ore, carbon dioxide and coke. If the heat contained in the carbon monoxide is more completely intercepted before passing out of the furnace, then, as we have seen, more of the ore is reduced and more of the monoxide is burnt to dioxide. But, on the other hand, if the temperature of the carbon monoxide be still further increased by the use of a still hotter blast, and if the heat of the up-flowing carbon monoxide be yet more completely intercepted before the gas issues at the mouth of the furnace, then we find that the temperature of the other materials is raised so far that the incandescent coke reduces the carbonic acid to carbon monoxide as fast as the ore oxidizes the monoxide to carbon dioxide. Consequently we have two opposite reactions which balance one another under certain conditions. When the temperature rises so much that the reduction of carbon dioxide is more rapid than the oxidation of carbon monoxide, it is obvious that we have passed the point where the furnace works most economically, for the reduction of the carbonic acid is accompanied by an absorption of heat. It appears therefore, as the result of practical experience, that we must always be content to only half burn in the blast furnace a large proportion of the fuel, sending out a *mixture* of carbon monoxide and of carbon dioxide from the mouth of the furnace.

One of the first great improvements in blast furnace practice was that of heating the blast. A more recent improvement has been to utilize outside the furnace the burning of that proportion of the carbon monoxide the presence of which in the escaping gases is, as we have shown, a necessity. In the modern furnaces the mixed carbon monoxide and carbon dioxide are drawn off near the top of the furnace, instead of being allowed to come in contact with the air. The hot gases are only admitted to the presence of oxygen when they have been brought by pipes to the furnace where the air for the blast is heated. The combustion of the carbon monoxide not only heats

the air for the blast, but furnishes power sufficient to drive the blast engine. The arrangements for utilizing the formerly "waste" gases of the blast furnace have effected a saving of more than half the total heating power of the coke, a saving equal, in Great Britain alone, to about four million tons of coal per annum.

GRASSES.

By J. PENTLAND SMITH, M.A., B.Sc., *Lecturer on Botany at the Horticultural College, Swanley.*

THE exceedingly natural group of plants known as grasses constitutes a large part of the flora of the British Isles, and is found generally diffused over the surface of the globe, "from the utmost limits of phænogamous vegetation towards the poles, or on alpine summits to the burning plains of equatorial Africa." Some species arrest the traveller's attention by their wealth of numbers, some by the commanding height which they attain, and others by both features combined.

They may live for one, two, or many years; some spread by means of underground stems, which send out roots here and there to fasten the plant in the soil and obtain fresh supplies of nourishment. In this way, certain species become troublesome weeds, as the well-known couch grass (*Agropyrum repens*). The roots vary much in size; in this country, from compact tufts, as in the vernal grass (*Anthoxanthum*), to the enormously developed vertically descending shoots of the wheat, which find their way many feet down into the soil. The root of the embryo soon ceases to grow, and numerous roots are developed further up on the stem to take its place. These adventitious roots are characteristic of monocotyledons, or plants with one seed leaf, to which the Gramineæ belong, although they are not confined to this class. In Fig. I., *r*, these adventitious roots are seen; the primary root is also here, but even in this early stage is barely to be distinguished from its adventitious neighbours. Each fibril is furnished with a root cap, and at some distance behind is clothed with absorbent root-hairs.

FIG. I.—Germinating Oat.

1. Showing paleæ which remain attached to fruit; *i.p.*, inner palea; *o.p.*, outer palea; *c.*, caryopsis; *r.*, adventitious rootlets; *a.x.*, portion of axis of spikelet; *st.*, stem.

2. *Ibid*, with paleæ stripped off. The origin of the rootlets is clearly seen. *co.*, coleorhiza; *st.*, stem.

3. Other side of 2, showing origin of stem; *st.*, stem.

At the points where the subterranean runners, or stolons, send off roots aërial stems also arise. These places are called nodes, and the portions between internodes. Nodes and internodes alternate in the same manner in the aërial stem from whose nodes leaves only are normally developed. Excepting at the nodes the aërial stems are hollow, and there they are characteristically jointed. The nodal septa, or divisions, are formed by the growing together of the vascular bundles which, as continuation of the veins of the leaves, run down the stem parallel to one another and to the periphery. The sedges (natural order Cyperaceæ) are nearly related to the grasses, and some of them are very similar in appearance to their allies. The tubular stem of the grasses and the solid stem of the sedges is, however, a point of distinction easily noted. It is true that in hot regions a few grasses, like the sugar-cane (*Saccharum*), have solid stems, and that the stolons of our British grasses are not fistular, but a hollow aërial stem is characteristic of all British species.

Grass stems are always very slender. A remarkable instance of this is found in the bamboo, which may grow to a height of fifty feet, although the stem is only five inches in diameter. The hollow nature of the structure and the development of its strengthening tissue towards the circumference render it able to resist effectively the strains to which it is subjected. The result of the non-development of the normal strengthening tissue, or sclerenchyma (σκληρος, hard, and εγχυμα, a tube), is sometimes seen in a field of closely sown wheat, especially if the crop be a heavy one and the season wet. The lower portion of the stem is so shaded that assimilation is almost quite prevented, and in consequence that portion is deficient in sclerenchyma, resulting in the "laying" of the wheat. A large amount of silica is present in some grasses, and this was considered to be the strengthening material. "The thoughtless assumption that the rigidity of the haulms of cereals is essentially promoted by the silica which they contain impelled agriculturists, thirty years ago, to manure their wheat fields with costly preparations of silica, hoping thereby to prevent the laying of the wheat."* Large deposits of silica occupy the internodes of the bamboo. This substance, known as tabascheer or tabaxir, possesses peculiar optical properties.

FIG. V.—Panicle of *Aira cæspitosa.*

The leaves are arranged on the stem in two rows. They have no petiole or leaf-stalk, but the leaf-sheath, which is split down one side, is well developed, and completely embraces the stem. Where it joins the blade or lamina, there is a tongue or ligule, generally of a scarious nature. The veins of the leaves, as in monocotyledons generally, run parallel to one another.

The flowers are arranged indefinitely on their axis, the older flowers being below, the younger above. One flower, or two or more arranged closely together on one stem, form a spikelet or little spike,† and there are all gradations from a spike of one-flowered spikelets, as in the Timothy

* Sachs, *Physiology of Plants*, page 289.

† A spike is an indefinite or racemose form of inflorescence, in which the main axis is elongated and the individual flowers sessile. A raceme only differs from it in having stalked flowers.

grass (*Phleum pratense*), to a compound loose raceme or panicle of many-flowered spikes, as in *Aira caespitosa*, *Avena* (the oat), and *Holcus lanatus* (the Yorkshire fog). Fig. V. represents the panicle of *Aira caespitosa*, Fig. IV. a spikelet of *Holcus lanatus*, and Fig. III. that of the perennial rye-grass (*Lolium perenne*), whose inflorescence is a spike of many-flowered spikelets.

We will now examine the two spikelets figured. At Fig. IV., *st* is the short stalk of the spikelet, by which it is fastened to a lateral branch of the main axis of the inflorescence. It bears first two leaves (*g. g.*), called the glumes; in the young condition these enclose the flowers borne on its upper portion. One of them is furnished with an arista or awn (*a*). There are two flowers in this spikelet. Each arises in the axil of a leaf called the outer palea (*o. p.*), which is morphologically a bract, as the definition of a bract is "a leaf in the axil of which a single flower arises." In dicotyledons — for instance, in Leguminosæ —two small greenish bodies may sometimes be observed, situated laterally on the peduncle or flower stalk, and in monocotyledons one placed opposite the bract, their number thus coinciding with that of the cotyledons. These structures are called prophylls or bracteoles. What is commonly called the inner palea (*i. p.*) of the graminaceous flower is thus a prophyll. The outer palea often bears an awn. In *Holcus lanatus* the outer palea* of the upper flower only is furnished with this appendage, and it arises from the middle of the back.

The structure of the flower is peculiar, and departs much from the typical monocotyledonous type. In the Liliaceæ (lily order), which may be selected as the typical form of this class, there are five whorls of floral organs—the perianth (περι, around, and ανθος, a flower) or floral envelope, two whorls of three members each; the andrœcium (ανηρ, a man, and οἰκος, a house), two whorls of three stamens each; and the gynœcium (γυνη, a woman) of three carpels united together. The members of the successive whorls alternate with one another, and the odd sepal is anterior, that is, it is situated next the observer who holds the flower between himself and the axis on which it arises. This arrangement is seen in Fig. II. 1. In the Liliaceæ there is a conspicuous perianth denoting that the flowers of that order are indebted to insects for performing the work of cross-fertilization. The flowers of the Gramineæ are wind pollinated, or anemophilous (ἄνεμος, the wind, and φιλεω, to love), and as is usual in such cases the perianth is inconspicuous, being here represented by insignificant, generally microscopical, scales, usually two in number, termed lodicules (Fig. II., *l*). These swell and push the bract outwards when the stamens and stigmas are ripe, and thus expose the organs of reproduction for the purpose of pollination. The two lodicules alternate in position with the outer palea: if a third one is present it stands opposite the inner palea. The stamens (Figs. III. and IV. *a n*) are generally three in number. Their very fine filaments appear to be fastened to the back of the anthers—versatile condition—owing to the downward growth of the anther lobes. The odd stamen is anterior; it is thus the inner whorl that is undeveloped. The two carpels (Figs. II. and IV. *c*) are united together or syncarpous (συν, and καρπος, a fruit). The two large feathery stigmas (Fig. III., *st*) are admirably suited to intercept the pollen grains wafted thither by the wind.

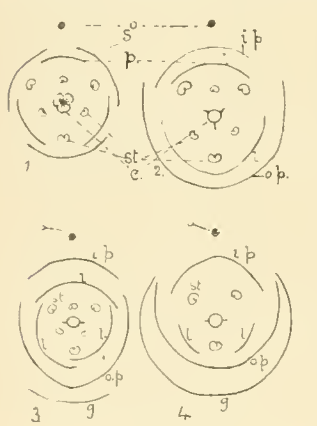


Fig. II.

1. Floral diagram of the Liliaceæ: *o.*, floral axis; *s.*, sepals; *p.*, petals; *st*, stamens; *c.*, carpels.

2. Ditto of Bamboo (*Bambusa arundinacea*); *l.*, lodicules; *o. p.*, outer palea; *i. p.*, inner palea.

3. Ditto of Rice (*Oryza*); *g.*, glume.

4. Ditto of Oat (*Avena sativa*).

A nearer approach to the structure of the typical monocotyledonous flower is made by that of the bamboo (Fig. II. 2), which possesses six stamens and carpels, but only three instead of six perianth segments. Six stamens are also present in some half dozen other genera, while in *Microlæna*, *Tetrarrhena*, and *Anomochlea* there are four. But two are found in *Anthoxanthum*—*Anthoxanthum odoratum* is the sweet-scented vernal grass—and one in Uniola and Monandraia (Fig. IIa., 5). These are thus more anomalous in form than Lolium and Holcus. Holcus, however, presents the anomaly of having hermaphrodite and male flowers on the same plant, a condition known as andro-monœcious (μονος, one)—see Fig. IV., where the lower flower of the spikelet is hermaphrodite, the upper male. Andromonœcism is also found in some members of the Fescue tribe. The rice (*Oryza*) has six stamens like the bamboo, but has only two carpels. In the maize (*Zea Mais*) the flowers are male or female, and both kinds are found forming female spikes and a male panicle on the one plant. The maize is thus monœcious. A curious change occurs in the female flower of Job's tears (*Coix lachryma*), a native of tropical Asia. The outer glume becomes bony and completely invests the mature spikelet, making the whole appear like a seed with a very hard case. The diagram of this anomalous form (Fig. IIa., 6) shows the position of the parts.

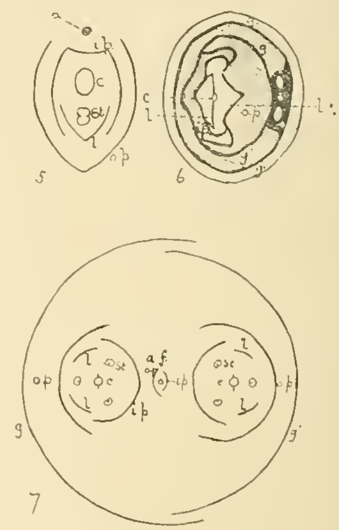


Fig. IIa.

5. Floral diagram of *Monandraia glauca*.

6. Ditto of Job's Tears (*Coix lachryma*); *g*, outer glume, forming bony involucre investing the spikelet; *g'*, inner glume. (After Le Maout and Decaisne).

7. Spikelet of Oat; *a. f.*, aborted flower.

Fig. IIa., 7, is a diagram of a spikelet of the oat (*Avena*).

The fruit is usually composed of two carpels, which enclose a single cavity containing a single ovule. It is generally the case that the seed or seeds are free from the wall of the ovary, but in grasses they are united together, and the resultant dry fruit, which does not open to allow its seeds to escape, is called a *caryopsis*. A longitudinal section of the maize fruit taken through the centre of the embryo exhibits the parts shown in Fig. IV. The pericarp (περι, around, and καρπος, a fruit) is formed from the united nucellus or wall of the ovule, and true pericarp or ovarian wall. The embryo occupies a very small portion of the cavity of the ovule, the remainder of the embryo

* The outer palea has also been termed the flowering glume, and the glumes the outer glumes. As a rule, the latter organs do not bear flowers in their axils.

sac being filled with nourishing matter called endosperm (ἐνδο, within, and σπέρμα, a seed). Only a small part of

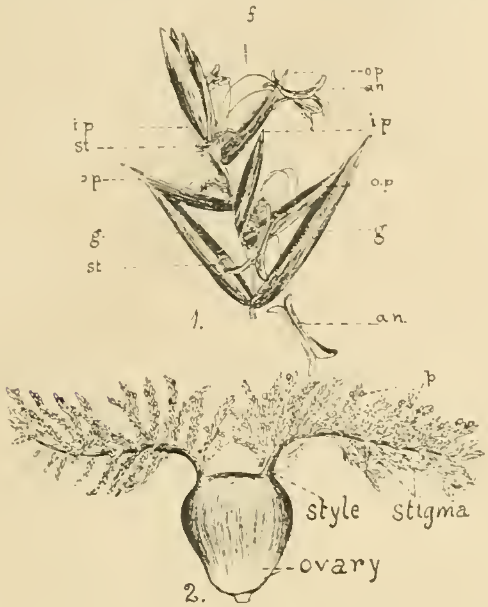


Fig. III.

1. Spikelet of the Perennial Rye Grass (*Lolium perenne*); *f.*, filament; *an.*, anthers; *i. p.*, inner palea; *o. p.*, outer palea; *st.*, stigma; *g.*, glume.
2. Gynœcium of *Lolium perenne*.

the endosperm is shown in the figure, but enough is portrayed to show that it is differentiated into a yellowish and a whitish portion, the former rich in nitrogen (albuminoid), the latter starchy. The embryo is composed of two main portions, the radicle and plumule. The lower portion of the radicle is the primary root, and the upper part the hypocotyl (ὑπό, under) or portion below the cotyledon or seed leaf. The plumule is the epicotyledonary part covered by its young leaves. This will afterwards form the greater part of the stem of the plant. The growing apex of the plumule is seen at *ap.* The embryo lies outside the endosperm, but it is attached to it by a large shield-shaped development of the hypocotyl, which at the same time nearly encloses the embryo itself. The epithelium of the scutellum, as this extraordinary structure is called, secretes ferments which digest the nourishing matter of the endosperm, changing the insoluble starch into soluble sugar, and the insoluble nitrogenous matters into soluble forms. These chemical changes commence when the mature fruit is placed in a moist medium, supplied with oxygen, and kept at a proper temperature. The soluble nutritive substances are transferred to the embryo plant by way of the scutellum, which thus acts like a placenta, and the seed commences to germinate. The radicle is pushed out of the fruit and the primary root penetrates the soil. Then the plumule makes its appearance, but the scutellum still remains attached to the endosperm, so that the seedling is still parasitic on the

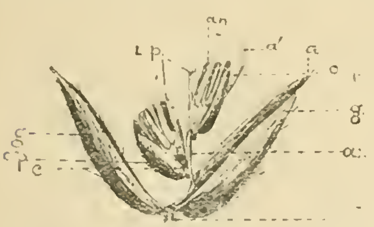


Fig. IV. Spikelet of Yorkshire Fog (*Holcus lanatus*); *ax.*, stalk of spikelet; *ax'.*, axis of flowers of spikelet; *g.*, glume; *o. p.*, outer palea; *i. p.*, inner palea; *a.*, awn of glume; *a'.*, awn of outer palea of upper flower; *an.*, anthers; *c.*, gynœcium.

seed. Gradually the endosperm is used up, and by this time the roots have penetrated the soil and chlorophyll bearing leaves have so developed as to enable carbon assimilation to proceed sufficient for the needs of the seedling. The young plant can then elaborate food material for itself.

The delicate apex of the root is protected by a cap of cells (*r. c*). The roots of grasses are enclosed in a sheath (*r. s*), which they break through in germination, and which then surrounds their base like a collar. It is called the *coleorhiza* (ῥίζα, a root). This structure occurs in some other monocotyledons and in a few dicotyledons. Adventitious roots are developed from the hypocotyl, and rupture the enclosing tissue, which remains like a collar, as in the case of the root-sheath. The primary root of the maize remains for a comparatively long period much larger than the secondary roots. In the oats, at a very early period of germination, it is a matter of some difficulty to determine the primary root (see Fig. I.). But in any case the primary root soon dies away, and the development of a large number of adventitious roots produces the fibrous roots common to the majority of monocotyledonous plants.

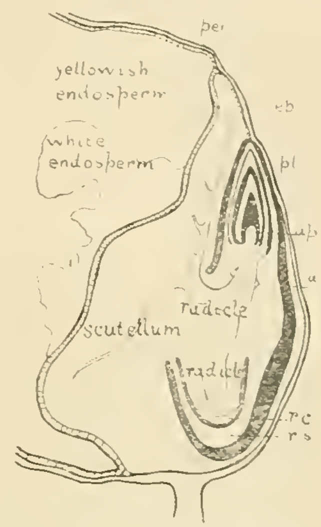


Fig. VI. Longitudinal section of Maize fruit, showing only a portion of the endosperm. *ep.*, epithelium of scutellum; *per.*, pericarp, composed of wall of ovule (nucellus) and ovarian wall united together; *pl.*, plumule; *ap.*, apex of stem; *a. r.*, adventitious root; *r. c.*, root cap; *r. s.*, root sheath.

The products derived from grasses are extremely various, and the plants furnishing these are very numerous. The most familiar examples of economical products are derived from the cereals—wheat, barley and oats. The origin of the two former is shrouded in mystery. They have both been cultivated in prehistoric times, and consequently to establish the home of the wild species in each case is a matter of great difficulty. "Very ancient Egyptian monuments, older than the invasion of the Shepherds, and the Hebrew scriptures show this (the wheat) cultivation already established, and when the Egyptians or Greeks speak of its origin they attribute it to mythical personages, Isis, Ceres, Triptolemus." * De Candolle also states that the Chinese, who cultivated wheat in B.C. 2700, considered it a gift direct from heaven, and that it is one of the five species of seeds annually sown in the ceremony of sowing five kinds of seeds—rice (*Oryza*), sorghum (a kind of millet), *Setaria italica* (another kind of millet), and soy being the others. He considers the Euphrates valley to be the probable home of the species. The account given by the same authority of the supposed mummy-wheat is worth quoting. "No grain taken from an ancient Egyptian sarcophagus and sown by horticulturists has ever been known to germinate. It is not that the thing is impossible, for grains are all the better preserved that they are protected from the air and from variations of temperature or humidity, and certainly those conditions are fulfilled by Egyptian monuments; but, as a matter of

* De Candolle, *Origin of Cultivated Plants*, page 354.

fact, the attempts at raising wheat from these ancient seeds have not been successful. The experiment which has been most talked of is that of the Count of Sternberg, at Prague. He had received the grains from a trustworthy traveller, who assured him they were taken from a sarcophagus. Two of these seeds germinated, it is said: but I have ascertained that in Germany well-informed persons believe there is some imposture, either on the part of the Arabs, who sometimes slip modern seeds into the tombs (even maize, an American plant), or on that of the *employés* of the Count of Sternberg. The grain known in commerce as mummy-wheat has never had any proof of antiquity of origin."*

Barley (*Hordeum vulgare*) may probably belong to Mesopotamia, but there exists a doubt with regard to the matter. De Candolle believes that all species of oats (*Avena*) cultivated are derived from a prehistoric form, and a native of eastern temperate Europe and Tartary. The common species is *Avena sativa*, of which, as in the cases of wheat and barley, there are numerous varieties in cultivation.

The most wonderful of all the grasses is the bamboo (*Bambusa arundinacea*).† It may attain a height of even eighty feet, and the rapidity with which it grows may fairly be said to be extraordinary—sometimes two to two and a half feet a day. The young shoots are eaten by the Chinese as we eat asparagus. Water buckets and water bottles are made from its stem if the joints be large enough. The stem is also used as timber in a variety of ways; and finer kinds of paper are manufactured by the Chinese from the inner portion of it. It can also be used for masts, and for agricultural and domestic implements, and we are familiar with it in the form of umbrella handles and walking sticks. A writer in the *Gardeners' Chronicle* of July 25th, 1891, records a remarkable phenomenon he witnessed in the province of Malabar—the seeding of the bamboo. "Hundreds of square miles were thickly covered with graceful clumps of this plant, many specimens being from sixty to seventy feet high. The leaves left the lateral branches and at the same time the inflorescence made its appearance, changing the aspect of the country as if by magic. No one was prepared for such an eventuality, and the English planters in the district were struck with something akin to dread when the fact dawned upon them that, in the course of a very brief period, not a living bamboo would be left in the forest. A few there were who refused to believe that the culms would perish after ripening their seeds, and were only persuaded by the actual realization of the fact." In the middle of May the seed came to maturity, and then there only remained the dead stems where once was a leafy forest of green waving plumes.

Of *Saccharum officinarum*, the sugar-cane, another interesting member of the Gramineæ, want of space prevents us speaking.

* De Candolle, *Origin of Cultivated Plants*, page 362.

† There are other species as *Bambusa verticillata*, etc.

THE REV. JOHN MICHELL, ASTRONOMER AND GEOLOGIST.

By John Richard Sutton, B.A.Cantab.

The world of to-day has almost forgotten John Michell, but no more bold and original thinker ever devoted himself to the study of the earth or the heavens. He lived at a time immediately preceding the advent of William Smith and Sir William Herschel, and his greatness has been kept in the background by the honour which the world has awarded to his successors. His name is mentioned occasionally, in authors' footnotes chiefly; and some later writers—notably Sir Charles Lyell, Prof. Grant and Arago—give him a share of commendation, though with little enthusiasm.‡ Of all the great cyclopædias, the *Encyclopædia Britannica* alone deigns to mention him at all, and even then incorrectly, though every one of them gives whole pages to lesser men. To a certain extent this is due to the fact that Michell's speculations were a full century before his age.

Michell was entered at Queen's College, Cambridge, and graduated fourth wrangler in 1748§—a position scarcely worthy of his powers.

In many ways there is something incongruous in associating Michell and his severe mental characteristics with the ivy-clad cloisters of Queen's and their memories of Erasmus and Ridley. Newton at Trinity, Wordsworth at St. John's, Young at Emmanuel, Cromwell at Sidney Sussex, where altar-candles are still waging an unequal battle with the persistent spirit of the old Puritanism—these are all natural figures in keeping with their college surroundings. But if college walls breathe inspiration at all, then assuredly neither Art nor Nature ever intended Queen's to be the abode of the hard and realistic sciences.

All the same, Michell flourished at Queen's. He took holy orders, and was elected to a fellowship immediately after taking his degree. He was elected Prælector Hebraicus and Prælector Arithmeticus in 1751, Censor Theologicus and Examinator in 1752, Prælector Geometricus in 1753, Censor Theologicus in 1754, Prælector Græcus in 1755. From 1756 to 1759 he was Senior Bursar (Thesaurius Superior). In 1762 he was again Prælector Hebraicus.‖

In 1750, only two years after taking his degree, he published his first work, an interesting little book of eighty-one pages, entitled "A Treatise of Artificial Magnets, in which is shown an easy and expeditious *method* of making them superior to the best *natural* ones, and of changing or converting their *poles*"—a most charming little volume, evidently the production of a mind above the common. Some of the experiments he describes indicate almost an instinct for tracing the workings of Nature, and are none the less interesting because of his most fascinating command of language. In this respect, indeed, Sir John Herschel was scarcely his superior.

In 1760, Michell was elected to the fellowship of the Royal Society, and in this year he published a paper on the cause and phenomena of earthquakes. The paper, which is very lengthy, is what such a scientific discussion should be; first a collection of facts, and then intelligent reasoning upon them. Certainly Michell shows, at this early stage of his career, a power of reasoning upon his facts which, up to that time, had been unknown in the history of geology. No available sources of information were neglected. The spirit of Epicurus was in the man tenfold. His words, to modern readers, convey the idea of prophecy fulfilled, rather than of deductions likely to have much weight at the time. "Some of his observations anticipate in so remarkable a manner the theories established forty years afterwards, that his writings would probably have formed an era in the science, if his

‡ It is due, however, to Mr. Ranyard to acknowledge that in the *Old and New Astronomy* he has spoken of Michell's work in a most appreciative way.

§ Also M.A. in 1752; B.D. in 1761.

‖ The author is indebted to the Rev. J. H. Gray, M.A., Dean of Queen's College, for these facts.

researches had been uninterrupted," as an eminent man of science admitted.* The whole paper should be studied.

When the Woodwardian Professorship of Geology at Cambridge became vacant in 1762 it was rightly felt that Michell was the man for the post, "and there is every reason to believe that had he retained the office for any length of time he would have done much to rescue the title of Woodwardian Professor, as well as the museum, from the species of contempt in which both were long allowed to remain. Unfortunately, however, after two short years he took leave of Cambridge and the Woodwardian possessions, attracted by the superior charms of a wife and a living."†

Of these two crowning blessings the wife came first, the living not until the end of another three years; for after 1764 Michell is not heard of at Queen's, and the natural inference would be that he vacated his fellowship in order to marry. It was obviously no flighty marriage, since he was pretty nearly forty years of age at the time and his wife seven years younger. Fortunately, marriage and his living did not withdraw him from scientific work, for the period between 1764 and 1767 (the latter year dating the commencement of his residence at Thornhill) was one of the most fruitful periods of his life.

In 1765 he was one of a committee of six ("three gentlemen skilled in mechanics and three watchmakers"), appointed by the Commissioners of Longitude, to examine the chronometers then recently invented by John Harrison. An Act of Parliament had been passed in 1714, offering for an exact chronometer a reward of "£10,000 if it determine the longitude to one degree of a great circle, or sixty geographical miles; £15,000 if it determine the same to two-thirds that distance; and £20,000 if it determine it to one-half that distance"; adding, "that one-half of such reward shall be due and paid when the said Commissioners, or the major part of them, do agree that any such method extends to the security of ships within eighty geographical miles from the shore, which are places of the greatest danger; and the other half when a ship by the appointment of the said Commissioners, or the major part of them, shall sail over the ocean from Great Britain to any such port in the West Indies as those Commissioners, or the major part of them, shall choose or nominate for the experiment, without losing her longitude for the limits above mentioned." After many delays and much bickering from Harrison, who, as a practical man, had strong objections to "men of theory," the committee was made up of "the Rev. John Michell, late Woodward Professor of Geology, the Rev. Will Ludlam, Fellow of St. John's College, Camb., John Bird, mathematical instrument maker, with Thomas Mudge, William Matthews, and Larcum Kendal."‡

A better man than Michell could not have been selected for this examination, and it is pretty clear that the committee as a whole did their duty very well. Harrison's chronometer was found on trial to keep the time so accurately as to allow the longitude to be ascertained to within one quarter of a degree, a perfection not anticipated by Parliament. The clever but slightly over-reaching watchmaker at once thought himself entitled to the prize, without disclosing the principle of his instrument, a consideration which would have completely nullified the purpose of the Act. Then the bickerings commenced afresh—Michell, and those with him, rightly declining to recommend the award unless the whole plan and construction of the chronometer were forthcoming; Harrison, whose temper was none the more docile for certain shabby treatment he had received from the Government, declining to divulge it, claiming that he had done all that the law required in *making* the instrument—and it was not until many words had been wasted on all hands that he could be persuaded to give way, to the great profit of navigation.§

Michell next appeared before the scientific public with two useful papers on the use of Hadley's quadrant for surveying and pilotage, and on a new method for measuring degrees of longitude, respectively, neither of which, however, need be explained here.‖

Finally, before settling down as a country parson, he published "An Inquiry into the probable Parallax and Magnitude of the Fixed Stars, from the quantity of light which they afford us, and the particular circumstances of their situation."¶ A finer paper has probably never been read before a scientific assembly—indeed, modern stellar astronomy has not got so very far beyond the verification of the conclusions it set forth. First, in a course of elegant arguments, Michell shows that the nearest fixed star is not very likely to be more remote than 220,000 times the radius of the earth's orbit. His procedure is simple, so simple indeed as to make one wonder why it was not thought of before. He compares the total light received from the sun with the average amount received from the first magnitude stars. To do this with absolute accuracy it would be necessary in the first place to obtain the actual amount of light received from the brightest stars by some such method as the use of, say, Prof. Pickering's photometer, or a wedge, and then to compare this with the light of the sun. But this would be a most laborious process, and not to be lightly undertaken; nor, in truth, were any such accurate means of light-measurement then known. Yet, even though the days of wedge and meridian photometers had not come, Michell could still show a very short and simple way of dealing with the question. Consider the planet Saturn when in opposition, and when both Saturn and the earth are at their respective mean distances. In this case, it takes but a rough observation to show that Saturn is not much brighter than the average first magnitude star. Take away the planet's ring and they will be nearly equal. Now, assuming that Saturn reflects *all* the sun's light which falls upon him—an assumption not strictly true, but near enough—and combining this with the consideration that the light received from a bright area varies inversely as the square of its distance, we have the following easy calculation:—The intrinsic brightness of Saturn is to the intrinsic brightness of the sun as the square of the distance of the sun's surface from its centre is to the square of Saturn's distance from that centre. Now, Saturn is about 2082 times further from the sun's centre than the sun's surface is; therefore the brightness of Saturn is to the brightness of the sun as unity is to 2082×2082. Again, the sun's diameter is about 105 times the diameter of Saturn; hence the sun's disc will be 105×105 times greater than Saturn's. And to

* Sir Charles Lyell: *Principles of Geology*, 9th ed., p. 41.

† From an historical sketch of the Woodwardian Museum in Smith's *Cambridge Portfolio*, by David Ansted, formerly Fellow of Jesus College. Sir David Brewster has observed that Michell held the Woodwardian Professorship for eight years, and Lyell repeats the statement. But Michell could not have held it for more than two years, for in 1765 he is spoken of as *late* Woodwardian Professor; nor would he have been allowed to hold that office and live at Thornhill at the same time. It is said that we owe to Michell many of the terms now in use among geologists, such as "Gault," "Coal-measures," &c. See also Woodward's *Geology of England and Wales*.

‡ Dodsley's *Annual Register* for 1765.

§ *Ibid.*

‖ *Phil Trans.* for 1765 and 1766.

¶ *Phil. Trans.*, Vol. LVII., p. 234.

compare the actual amount of light sent out by each, we have to multiply the numbers representing the areas by the numbers representing the intrinsic brightness. Making the calculation, we see that the light of the sun is to the light of Saturn as $2082 \times 2082 \times 105 \times 105$ is to unity. Now, the square root of the product just given is 218,210; which means that we should have to remove the sun to about 218,210 times its present distance before it would shine as a first magnitude star.

This, of course, is only an approximation, and only useful (but very useful, nevertheless) in framing first and elementary ideas as to the extent of the visible universe. Michell was careful to point out that although his method might be trusted to give trustworthy *general* data to start with, it would not be applicable to special cases; for, as he warns us with one of his touches of inspiration, most likely the stars are not by any means of an equal size, and the surface brightness may differ very greatly in different stars.

The paper then goes on to discuss methods for determining the actual sizes of the stars, and the difficulty—nay, the impossibility—of ever solving this great problem: "There seems to be little chance," says Michell, "of discovering with certainty the real size of any of the fixed stars, and we must consequently be content to deduce it from their parallax, if that should ever be found, and the quantity of light which they afford us compared with that of the sun. And in the meantime, till this parallax can be found, or something else may arise to furnish us with a more general analogy, we can only suppose them, 'at a medium,' to be equal in size to the sun, this being the best means which we have at present of forming some probable conjecture concerning the extent of the visible universe. That we may be the better enabled to do this, it seems to be an object worth the attention of astronomers to inquire into the exact quantity of light which each star affords us separately when compared with the sun; that instead of distributing them, as has hitherto been done, into a few ill-defined classes they may be ranked with precision, both according to their respective brightness and the exact degree of it."

Let us pause for a moment to see what modern astronomy has to say to Michell's results. Seventy years had to pass before Mr. Henderson and Sir Thomas Maclean could prove to the world from observations on the parallax of Alpha Centauri that Michell's theoretical estimate was not far from the truth. Moreover, the qualifications he had laid down as to the various sizes of the stars were also shown to be justified by the results of a comparison between the discoveries of Henderson and Maclean, and those of Bessel on the star 61 Cygni; for this latter star, although only about three times as far away, gives us scarcely a two-hundredth part of the light of Alpha Centauri, whereas if all the stars were equal in size and brightness it would send us nearly thirty times as much light as it does. If it should be urged that Michell's speculations were nothing but lucky guesses, it may be retorted that such luck comes only to genius. For the rest, it may be worth noting that Michell's dream of a star-catalogue, in which all the stars should be "ranked with precision," has only begun to be realized within the last twenty years or so.

The best part of Michell's extraordinary paper is his application of the theory of probabilities to the distribution of the stars. It has always been the custom to divide the stars into groups or constellations, for religious or historical purposes, or for convenience of reference. Generally speaking, these groups or constellations are well-defined: Orion, Ursa Major, and the Pleiades, for example. From the time of Copernicus this grouping had been regarded as accidental, and the constellations were supposed to result from the projection of a number of bright stars, differing greatly in distance, upon small portions of the imaginary sphere of the heavens. Michell then took up the parable, and applied the doctrines of probability to the question for the first time in the history of astronomy. Dealing with the brighter stars in the Pleiades, as being the most convenient for his purpose, he was able to show that the odds were 500,000 to one against any six physically unconnected stars scattered at random through space being found projected upon the sky so closely as these are.* Hence it is nearly certain that the Pleiades (and other similar groups, of course, by the same reasoning) are in reality physically, as well as optically, associated. Unfortunately, this grand result, so valuable in itself, and so full of promise to further research along the same lines, was practically neglected, and astronomers took over a century to rediscover that which Michell with good reason regarded as demonstrated. The observations of Bessel, Wolf, and Elkin shows that the greater number of stars in the Pleiades group have a common small proper motion, which seems to indicate that the whole group is a connected system; and the more recent application of photography has confirmed this theory by revealing the remarkable fact that a gigantic nebula envelopes the whole or the greater part of the whole group. An additional and strong argument was also advanced on the same side by the late Mr. Proctor, founded upon the small number of stars in the sky-space adjacent to the Pleiades, suggesting, so great is the contrast, a sea of darkness enclosing an island of light. Surely, if the greatness of Michell needed demonstration these facts ought to be enough. It may be added that the laws of probability apply in exactly the same way to cases where two, or ten, or any number of stars are seen close together on the sky, though the odds in favour of a physical connection in such instances are much reduced as the number of stars so situated is less.

The remainder of the paper is taken up with a discussion of the relations presented by particular circumstances of star-grouping, and the possible position of the sun amidst the stars. The results in the main are nothing less than what astronomers during the last twenty or thirty years have begun to see clearly. But singular injustice has been done to Michell in this as in all things else. Recent writers have either appropriated his work without acknowledgment, or have too generously handed it over to others. So the intimation that stars optically close are almost certain to be physically connected has been placed to the credit of Sir William Herschel, who simply determined the orbital *motions* of some of these close stars, and describes himself as surprised at the discovery of their connection. In speaking of the discovery, he says that he went out like Saul to seek his father's asses, and found a kingdom—the dominion of gravitation extending to the stars. It had been known for a very long time that many of the stars which appear single to the naked eye are double or triple when seen in the telescope; and Michell's paper was a satisfactory proof of the significance of the phenomena. The great telescopic powers which Herschel used enabled him to split up a great many more stars which were not previously known to be double, but so far was he from grasping the physical explanation of his facts that, in his first catalogue (published in 1782), he sees no other reason for a change of position in the components of a double star than "by admitting a proper motion in either

* Sir John Herschel points out that when the stars of the southern hemisphere are included in the investigation these figures have to be considerably reduced. The correctness of the argument is unaltered, however.

one or the other of the stars, or in our solar system." But in 1784 Michell pointed out that the great and increasing number of known double stars could leave no doubt in any properly-balanced mathematical mind (even if his former paper, in spite of its lesser basis of knowledge, were not sufficiently convincing) "that by far the greater part, if not all of them, are systems of stars so near each other as probably to be liable to be affected sensibly by their mutual gravitation; and it is therefore not unlikely that the periods of the revolutions of some of these about their principals (the smaller ones being on this hypothesis considered as satellites of the others) may some time or other be discovered." * Very little notice seems to have been taken of this confident prediction. Twenty years passed, and the scientific world exclaimed, *How wonderful!* when Herschel's observations forced him to acknowledge that many of these double stars were really binary systems, whose components revolve about each other in the same way as the earth and moon.

All this is in Michell's one paper of 1767†. Had he never after put pen to paper, nor engaged in any further scientific pursuits, enough had been accomplished to place him in the front rank of English philosophers. Not only did he invent his own method of procedure in his researches, but as its result he confidently announced the solution of problems which men of that day almost thought it presumptuous to speak of. The theory of probabilities not being then appreciated and relied on as now, there seemed more of shadow than of substance in his speculations. More attention would have been accorded him by his contemporaries, perhaps, had he, like Herschel, done something sensational. Herschel attracted little attention until his discovery of Uranus burst upon the world. But Michell did nothing to take the world by storm.

(*To be continued.*)

* *Phil. Trans.*

† Mr. Sutton has not referred to what seems to me to be the most striking and remarkable suggestion in this paper, in which Michell anticipates Pickering's and Monck's methods of comparing the brightness or density of double stars. It is contained in a note on page 238 of his paper, and most concisely points out how, if we know the period and brightness of a double star, we may neglect its parallax, and directly compare its brightness and mass with that of the sun. Prof. E. C. Pickering, in 1880, showed how, in dealing with a binary star system, the parallax might be neglected, and the density or brightness of the double star might be compared directly with that of the sun. Mr. Monck, in the *Observatory* of 1887, showed how the brightness or density of one binary system might be compared with that of another binary system. Neither Pickering nor Monck seem to have known of Michell's note which anticipates their whole line of reasoning. It runs as follows:—

"If, however, it should hereafter be found that any of the stars have others revolving about them (for no satellites shining by a borrowed light could possibly be visible), we should then have the means of discovering the proportion between the light of the sun and the light of those stars, relatively to their respective quantities of matter, for in this case the times of the revolutions and the greatest apparent elongations of those stars that revolved about the others as satellites being known, the relation between the apparent diameters and the densities of the central stars would be given, whatever was their distance from us, and the actual quantity of matter which they contained would be known whenever their distance was known, being greater or less in proportion to the cube of that distance. Hence, supposing them to be of the same density with the sun, the proportion of the brightness of their surfaces, compared with that of the sun, would be known from the comparison of the whole of the light which we receive from them with that which we receive from the sun; but if they should happen to be either of greater or less density than the sun, the whole of their light not being affected by these suppositions, their surfaces would, indeed, be more or less luminous, accordingly as they were upon this account less or greater, but the quantity of light corresponding to the same quantity of matter would still remain the same.

"The apparent distances at which satellites would revolve about any stars would be equal to the semi-annual parallaxes of those stars, seen from planets, revolving about the sun in the same periodical times with themselves, supposing the parallaxes to be such as they would be if the stars were of the same size and density with the sun."

At the date of Michell's paper (1767) less than seventy close double stars were known, and none were known to be moving about one another, but in this paper, as well as in a second paper published in the *Phil. Trans.* for 1784, he speaks with confidence as to the physical connection between such closely situated pairs.—A. C. Ranyard.

WHAT IS A NEBULA?

By A. C. Ranyard.

THERE can be little doubt that the nebulæ are, as a general rule, very transparent, for it cannot reasonably be doubted that by far the greater number of the stars which appear to us surrounded by nebulous matter are really involved in the brighter and central parts of the nebulæ which appear to surround them, and are not merely seen by chance projected on a nebulous background. A very elementary application of the doctrine of chances will show the enormous improbability of the hundreds of nebulous stars known, all being seen projected (generally pretty centrally) upon a nebulous background. In the case of the great nebula in Orion, and of the Pleiades nebula, as well as in some of the smaller nebulæ, we actually see nebulous structures which appear to spring from stars or groups of stars like the trapezium in the Orion nebula; the nebulous structures grow gradually fainter as the distance from the star from which they appear to spring increases, and in some instances the nebulous structures branch or divide in a direction away from the star in a manner which leaves no room for doubting that the seat of origin of the structure (that is, the place from which it was belched forth) must have been within the star.

We may therefore feel practically certain that we receive the light from many stars after its passage through many thousands of millions of miles of nebulous matter. We know how the light of our own sun is dimmed at sunset and sunrise by its passage through a few hundred miles of our atmosphere, so that the eye can easily gaze on the sun's disc; and a photograph which at midday can be obtained in a fraction of a second, takes at sunset or sunrise many seconds, or even some minutes, to give it a suitable exposure. The almost perfect transparency of this nebulous matter will be best realized by the student of physics who knows that if half the light were absorbed in its passage through ten million miles of nebula, only a quarter of the light radiated by the star would get through twenty million miles of similar nebulous matter, and only an eighth part through thirty million miles, only a sixteenth through forty million miles, and so on; for each succeeding ten million miles of similar nebulous haze would halve the light which had succeeded in getting through the nebulous veil between it and the source of light. Thus at a distance of 200 million miles from the nebulous star the star's light would be reduced to about one millionth of the emitted light; at a distance of 400 million miles it would be reduced to a millionth of a millionth of its original brightness; and after passing through 1000 million miles its light would have been reduced 75 magnitudes in the stellar scale of brightness.

Such vast distances are triflingly small when measured by the scale of architecture on which nebulæ are built. A nebula a thousand million miles in diameter at the distance of *α Centauri* (our nearest neighbour amongst the stars) would only appear to have a diameter of 8·12″ seconds of arc, while the nebula in Orion has a diameter of more than half a degree, and the great nebulous and stellar structure shown in our plate has a diameter of more than 20°.

There is another class of reasoning which enables us in a vague and rough way to fix a superior limit for the density of these nebulous masses. If we suppose the Orion nebula to be a uniform sphere of only a third of a degree (20′) in diameter, with an average density of only one millionth of our atmospheric air at the sea level, the mass of the nebula would be such that at the distance of *α Centauri*, assuming gravity to act across interstellar

space,* it would be capable of giving our sun a velocity of 255·2 miles per second if it fell towards the nebula from an infinite distance, and the velocity of our sun in a circular orbit about such a nebula, situated at a distance equal to the distance of *α Centauri*, would be 180·4 miles per second. If the nebula contained such a vast mass of attracting matter we should expect to see many stars in the neighbourhood of the nebula moving across the line of sight with very large proper motions, for a velocity of 100 miles a second across the line of sight at the distance of *α Centauri* would give an annual proper motion of 25·5″, that is a proper motion more than three times as great as that of 1830 Groombridge, which is the swiftest moving star which has at present been discovered.

But instead of finding the stars in the immediate neighbourhood of the Orion nebula exhibiting large proper motions, we find the stars in this region of the heavens, which appear to be associated with the Orion nebula, show hardly any detectable proper motion, and the same remark applies to all the stars connected with the stream of nebulæ which appear to link up the great Orion nebula with the Milky Way. The stars of the Pleiades group, which also appears to be connected with the Milky Way and to be surrounded by a very extensive nebula (see Knowledge for May, 1891), also exhibit only small annual proper motions, and the same remark applies to the nebulous star *α Cygni*, which appears to be associated with the nebulosity of the Milky Way (see Knowledge for October, 1891).

If, instead of assuming the distance of the Orion nebula to be equal to the distance of *α Centauri*, we had assumed its distance to be double as great, the velocities referred to above, of our sun in a parabolic orbit or in a circular orbit about the nebula would need to be doubled. For at twice the distance a nebula subtending the same angular diameter would occupy eight times the volume, and, the density remaining the same, its mass would be eight times as great, and the periodic time in a circular orbit about such a nebula at double distance would be unchanged, therefore the velocity in the larger orbit would be doubled.

That the periodic time is independent of the distance of the nebula will be evident when it is remembered that the square of the periodic time, in any orbit, is inversely proportional to the attracting mass. Therefore, if the central attracting mass be multiplied by eight, the size of the orbit remaining unaltered, the periodic time will be reduced in the proportion of 1 to $\frac{1}{2\sqrt{2}}$. On the other hand, if it is evident from Kepler's law connecting the squares of the times with the cubes of the distances, that if the attracting mass remains unaltered, and the size of the orbit be doubled, the periodic time will be increased in the ratio of 1 to $2\sqrt{2}$; therefore, if the size of the orbit be doubled, and the attracting mass be multiplied by eight, the periodic time will remain unaltered,† and the velocity in the orbit will be doubled if we double the distance of the nebula.

If we assume the distance of the Orion nebula to be equal to the distance of *α Centauri*, that is, taking the parallax of *α Centauri* as 0·75″, or that it is situated at a distance of 274,900 times the earth's distance from the sun, and that the mean density of the nebulous matter is one hundred millionth of the density of atmospheric air at the sea level—that is, that the density of the nebula is about $\frac{1}{122,160,000,000}$th of the density of the sun—for water is about 846 times as heavy as an equal volume of air at the sea level, and the density of the sun is about 1·444 as compared with water; the mass of the nebula, supposing it to be spherical and to have an angular diameter of 20′ would be 330,200 times the mass of the sun, and the periodic time in a circular orbit about such a body at a distance equal to the distance of *α Centauri* from the sun would be 281,450 years, which corresponds to a velocity of a little more than 18 miles a second. If we suppose the nebula to be at double the distance of *α Centauri* the velocity in a circular orbit would be 36 miles a second, and so on, the velocity increasing directly as the distance of the nebula is increased.

We may probably feel quite sure, from the small observed proper motions in the neighbourhood of the Orion nebula, that its average density does not exceed one ten thousand millionth of the density of atmospheric air at the sea level. This would about correspond to the mean density of the solar nebulous mass, supposing it to have been spherical when its radius was a little more than 107 astronomical units, or when the sun occupied a sphere with a radius of a little more than $3\frac{1}{2}$ times the distance of Neptune.

In examining the forms of nebulæ we find comparatively few oblate spheroids, such as the hypothesis of La Place assumes. There are many apparently spherical masses, a few spirals and rings, and a great many nebulous masses of irregular form. If the stars we see are of very different ages, and the nebular stage of condensation occupies, as has hitherto been supposed, a very lengthy period compared with the stellar stage, we should expect to see a far greater number of nebular masses than of fully-formed stars, but the number of brightly shining stellar points greatly exceeds the number of nebular masses hitherto discovered. Possibly we are mistaken in supposing that the faintly shining nebular masses we observe afford ocular evidence of the truth of La Place's bold hypothesis. The nebulæ we see have, it seems to me, a greater analogy with the solar corona than with the fiery condensing mists conceived of by La Place; they are very generally associated with stars, and in some cases the nebulous structure clearly indicates that the nebulous matter has issued from the star, and sometimes from a starless region. The forms of nebulæ are certainly in general inconsistent with the theory that stars are condensing from nebulæ.

The dark tree-like structure which extends diagonally from the right lower corner of our plate up towards the left hand upper corner clearly tells the story of matter projected into a resisting medium, and not of nebulous matter slowly condensing under the influence of steady rotation. If the reader will take the trouble to compare two copies of the plate—one a dark print and the other a print in which the nebulosity is whiter—he will recognize several smaller tree-like forms extending laterally on either hand from the main trunk of the dark structure, as well as several streams of stars evidently springing from the dark region.

* In view of what we know of the motions of binary stars about one another, the philosopher who has serious doubts whether the action of gravity extends across interstellar space must have a very highly-developed organ of philosophic doubt. He must be willing to assume that while gravity extends apparently, according to law, across the space which separates the sun and Neptune, and across the probably still wider spaces which separate many of the binary stars, its action ceases or is interfered with in passing across the interstellar spaces which separate the systems within which gravity reigns.

† The above reasoning only holds when the mass of the sun may be neglected as compared with the mass of the nebula.

Letters.

[The Editor does not hold himself responsible for the opinions or statements of correspondents.]

THE ATMOSPHERES OF CELESTIAL BODIES.

To the Editor of Knowledge.

Sir,—In discussing the atmospheres of the moon or of Mars, I think we rely too much on the analogy of our own

The Region of the Milky Way about ζ Cephei.

Enlarged from a photograph taken by Dr. Max Wolf, of Heidelberg, with a portrait lens of 55 millimètres (about $2\frac{1}{8}$ inches) aperture, and 195 millimètres (about $7\frac{2}{3}$ inches) focal length; exposure 7 hours.

The Automatic Engraving Company, Strode Road, Willesden Green.

atmosphere, which consists mainly of permanent gases whose quantity is not altered by evaporation or liquefaction. With this is joined a much smaller atmosphere of aqueous vapour, which varies greatly with the conditions of time and place. But these variations would be much greater if the permanent gases were removed; for I think there is no doubt that their presence retards both evaporation and precipitation of vapour.

When a star is occulted by the moon, the disappearance is usually instantaneous; but to this rule there are many exceptions. It seems to me that both rule and exceptions can be explained by ascribing to the moon an atmosphere of vapour which never attains large dimensions, and is at many times and places wholly, or almost wholly, condensed. It need not, of course, be aqueous vapour. Oxygen and nitrogen would probably act in the same way if the temperature were sufficiently low, but it is not likely to be low enough for this purpose on the moon. The absence of cloud may be considered an objection to the theory of an atmosphere of vapour. But, as you pointed out in the last number, dust has a good deal to do with the formation of cloud, and so I believe has atmospheric air. In the absence of air and dust, precipitation would perhaps take place with little or no formation of clouds or fog.

The existence of water on the moon is doubtful. On Mars, however, it appears to be certain, and it is not unlikely that vapour plays a much more important part in the Martian atmosphere than in ours. On one point, however, we are at present wholly in the dark, viz., What is the effect of a diminution in the force of gravity upon water? We have only the power of studying this to a very limited extent on the earth, and I am not aware that it has been studied at all. From the marked effects of pressure on both the freezing and the boiling points of water it seems probable that variations in the force of gravity would affect these points also; but how, we can only conjecture. Water, however, might exist in the liquid state at a temperature far below freezing point if it were sufficiently salt. Again, supposing that Mars has no atmosphere of oxygen and nitrogen, the result would be to modify the properties of water considerably. Water when deprived of air becomes viscous, its boiling point is raised and it boils with explosive violence; even its visible appearance is altered.

Aqueous vapour, according to the experiments of Tyndall, has a very high absorptive power. This might account for the small reflection of light from the surface of Mars. If, moreover, it absorbs blue or green rays by preference, the red tinge of those reflected from the solid body of the planet would be accounted for. The red tinge of the eclipsed moon has sometimes been ascribed to a similar cause, the absorbing agent being the vapour in our own atmosphere. The atmosphere would probably be more dense in the equatorial than in the polar regions, but this would be modified by the distribution of water on the planet and the existence of mountains or high tablelands.

With regard to the *relative* albedo of different planets, it ought, I think, to be easy to detect. Supposing that the entire surface of the planet is illuminated (otherwise allowance can be made), the illuminated surfaces are proportional to the squares of the planets' diameters. If the whole of the incident light was reflected the intensity, for different planets, would vary directly as this square, and inversely as the square of the planet's distance from the sun multiplied by the square of its distance from us. If the relative lights thus computed for Jupiter and Mars differ from those directly observed, the difference is due to the relative albedo of the two planets. One planet should be taken as the standard and the albedo of each of the others expressed in terms of its albedo.

Truly yours,

16, Earlsfort Terrace, Dublin. W. H. S. Monck.

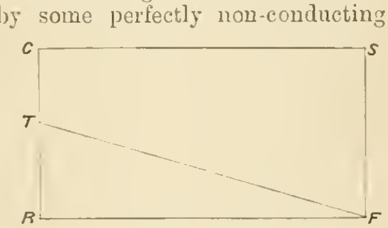
[The presence of air or other gases makes no difference in the amount of water that will be evaporated at a given temperature, but it makes a considerable difference in the rate of evaporation. Thus, if a pint and a half of water were brought into a room measuring ten feet each way, and filled with perfectly dry air, or any other gas or vapour, at a temperature of 80° Fahrenheit nearly the whole of the water would be evaporated, and the air would still remain transparent, and the walls of the room dry. When this amount of water had been evaporated the process would stop, and no more vapour would be given off unless the temperature of the room was raised. Experiments show that the same amount of water would be evaporated, at the same temperature, whatever amount of dry air there was in the room to start with; even though the room was devoid of air, the same amount of water would disappear, and the pressure of the resulting water vapour would raise a column of mercury in a barometer by about an inch. On Mars the same tension of water vapour would raise the column of mercury a little more than two and a half inches. I do not feel as sure as Mr. Monck or my friend Mr. Maunder as to the evidence with regard to the absorption of the sun's light by aqueous vapour in the atmosphere of Mars, and I am quite prepared to believe that the white polar caps of Mars are due to the white snow-like crystals of carbonic acid gas, or even to condensed atmospheric air.—A. C. Ranyard.]

CAN A PLANET ABSORB ITS OCEANS WITHOUT HAVING ITS SURFACE TEMPERATURE LOWERED?

To the Editor of Knowledge.

Sir,—In the June number of Knowledge, p. 114, the editor criticizes the suggestion that if Mars formerly had more extensive oceans than now, owing to internal cooling they might have been absorbed. He makes the point that in the case of the earth such an absorption could only occur in case the surface temperature, even in the equatorial regions, were reduced, owing to internal cooling, below the freezing point.

Now it seems to me that the internal temperature of the earth has no influence whatever upon our climate, and that if its internal temperature should be reduced to that of interplanetary space our surface temperature would still be practically the same as at present. Let us imagine an iron bar one thousand feet long and several feet in diameter, surrounded by some perfectly non-conducting substance. Let one end, CR, be maintained at a constant temperature of 1000° C., and let the other end, SF, be exposed to the outside air, which we will say has a mean temperature of 0° C. In the course of several days variations of temperature inside the bar will cease, and the temperatures at varying distances from CR can be represented by the ordinates of the line TF. Thus, at one-tenth of the distance from F to R, that is at one hundred feet distance, the temperature will be 100° very nearly.



If, instead of iron, the bar were made of clay we should have practically the same condition of affairs, only instead

of having to wait several days for a permanent condition of temperature to be established, we should have to wait several centuries. There would then still be one other difference between the two cases. Owing to the high conductivity of the iron, the surface SF would be a few degrees above the temperature of the outside air, while the surface of the clay would be almost exactly equal to it. This difference would depend upon the radiating capacity of the surface SF. In this respect our earth resembles the clay; in other words, the radiating and absorbing power of the surface SF so far exceeds the conductivity of the long bar of clay that the temperature of the surface depends practically entirely upon this radiating and absorbing power, and will be the same whether the temperature of CR is 0° C. or 1000° C.

Suppose now that the temperature of CR should be permitted to fall to 500°. Since the temperature of SF still remains at 0°, we should now be able to advance twice as far from F before we should reach the temperature of boiling water.

Applying this experiment to the case of our earth, let us assume that half of the free water of the planet is at present underground, and that all the microscopic cavities that are not too hot are already filled with water. When the internal temperature of the earth is lowered to one-half of its present figure, there will, other things being equal, be nearly twice as much space for the underground water, and as a result we may expect that our oceans will disappear, leaving our earth a warm but arid desert. Indeed, this might occur at an earlier date, since at the greater depths water would remain in the liquid form at a higher temperature.

William H. Pickering.

Arequipa, Peru,
August 5th, 1892.

P.S.—In printing my letter in the June number the word *if* has been left out; it makes a very material difference in my meaning. What I wrote was, "In the case of this planet (Mars), however, we have good reason for thinking that *if* it formerly had extensive oceans upon its surface, *that by* the gradual cooling to which it has been subjected there has been room formed for them in its interior."

[I agree with Prof. Pickering as to the increase in underground temperature being probably slower in the body of Mars than in the earth.

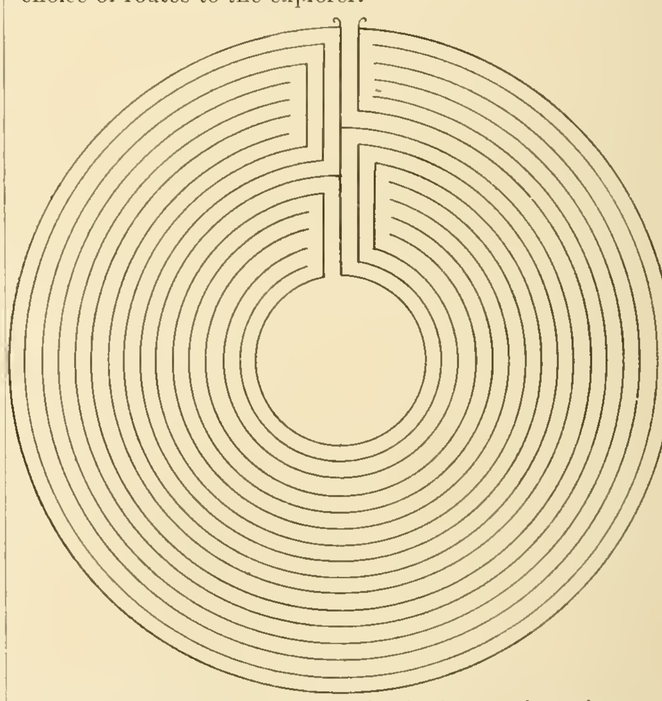
The great range in the mean temperature in passing from the earth's equatorial to its polar regions shows that if it were not for heat derived from the outside the whole surface of the earth would have a mean temperature below the freezing point of water. No amount of atmosphere could increase the heat of the surface derived directly* from the sun. One might as well expect to hasten the cooking of a joint before a fire by covering the joint with a glass shade or with a blanket, and we may be sure that the heat derived from the outside by the surface of Mars is, area for area, less than that derived by the earth's surface from the sun in a proportion which varies with the inverse squares of the distances of the two planets from the sun. If, therefore, as Prof. W. H. Pickering assumes, "the internal temperature of the earth has *no influence whatever* upon our climate, the mean temperature of even the equatorial regions of Mars must be far below the freezing point of water.—A. C. Ranyard.]

* Though an atmosphere which was transparent to short wave-lengths but absorbed long-wave lengths would tend to accumulate the heat of the surface by preventing the return from the earth's surface into space of long wave-lengths, corresponding to heat of low temperature derived from solar radiation of shorter wave-length.

THE CRETAN LABYRINTH.

To the Editor of Knowledge.

Dear Sir,—That there was some kind of a maze or labyrinth at Cnossus in Crete need not be doubted, as there are coins or tokens of Crete with the device of a labyrinth impressed upon them. Several of these coins are displayed in the glass cases accessible to the public in the British Museum. There is also, of course, the myth about Theseus entering the labyrinth by the help of a thread given him by Ariadne, when he slew the Minotaur or Man-Bull who lived in the centre, and put an end to the cruelties practised by the monster. This legend, beautiful enough when taken in the anagogic sense, was probably in existence before the maze shown on the coins took any definite shape, or it may be that several successive mazes were built and kept as long as any profit could be made out of those who came to visit them. I mention this because some of the mazes shown on the coins are circular, while others are rectangular, although the twists and turns are practically the same in both plans. Now, unfortunately, the labyrinths depicted on the coins are apparently of such a simple character that anyone could find his way to the centre and out again by the simple process of following his own nose, because the maze seems to consist of a roundabout path, leaving at no point any choice of routes to the explorer.



It has occurred to me that the device on the coins was never intended as a map or plan of the labyrinth, but only as a key or clue to the right path, leaving out the wrong ones. By taking the maze as shown on the Cretan coins, and treating the circular dividing walls as *double*, and each containing a passage of the same width as the road shown on the coins—in fact restoring the design to that shown on the diagram accompanying this paper—one can see that penetrating the maze to the very centre by the nearest road and at the first attempt would be a matter of very serious difficulty. Some of your younger readers will find it not quite easy to trace the white path with the point of a bone knitting needle; or, if they take the trouble to trace the maze large on the sea sand, they will find that their companions not in the secret will get a good deal of exercise before arriving at the centre. They must remember

too that they have the advantage of a bird's-eye view, which anyone entering a real structure made of walls or hedges would not.

I have at the principal doubling points merely left open two more of the roads than would have resulted from simply treating the dividing wall as another path. Though not necessary, this seems to give a little more variety to the plan.

It will be seen that there are four places where the right way has to be chosen between five, and as no one would be so stupid as to choose the road by which he had just entered, the chances are about three to one against his taking the right one of the other four. This difficulty is repeated four times in the course of the labyrinth, so that it would seem on a first calculation that his probability of getting in by the best route is about $\frac{1}{3}$ of $\frac{1}{3}$ of $\frac{1}{3}$ of $\frac{1}{3}$, or, say, 1 to 81. But any road will, after an uncertain number of turns and doublings, take him either to the centre or back again to the entrance; so that the task is not nearly so difficult as it would at first sight seem, nor, indeed, is it at all probable that the designer would have made it too difficult, especially if any body of men had to thrive on the profits, like the artificers who made silver images of Diana at Ephesus. Perhaps even the labyrinth coins took their origin from tokens, given to those who had visited the maze and duly paid for the privilege. They might also serve as a sort of pass token and souvenir of the visit, as well as a key to the mystery. The legend of the coloured thread of Ariadne becomes intelligible on this plan of the maze as now shown, because, on returning to any place where he had been before, Theseus would see his own thread and retrace his steps, gathering up the line until he could make a better start—while of course the thread would serve him as a guide to enable him to get out again. If I gave the coin plan, it would simply be each alternate circle of this one, and it would furnish a key by which anyone could immediately find the way to the centre.

The solution, when put into words, is—avoid every alternate circle or path.

Yours faithfully, Richard Inwards.

A FLAKE OF FLINT AND ITS HISTORY.

By R. Lydekker, B.A.Cantab.

In our article entitled "A Lump of Chalk and its Lessons," published in Knowledge for June last, it was stated that the upper white chalk, so far as hand specimens are concerned, is a nearly pure limestone. This, indeed, is a perfectly true statement as regards such hand specimens; but when we consider the upper chalk as a whole, we must not omit to regard the numerous bands and nodules of flint with which it is interstratified as a very important constituent of the whole rock. We say a constituent of the whole rock advisedly, because although the flint is now separated from the white limestone which we call chalk in the form of nodules and bands, yet there is evidence that it was originally disseminated throughout the entire mass, and that the upper chalk then formed a slightly siliceous limestone. Probably everybody is more or less familiar with flint as it occurs in a chalk pit, or in the form of gravel derived from the disintegration of chalk strata; but it may be taken for granted that comparatively few have ever seriously considered how the solid masses of flint have originated in the soft chalk limestone. As this is a subject of considerable interest, and one which has given rise to much discussion, we propose to devote the greater part of the present article to its consideration, while we shall add some observations on the history of flints after they have been removed from their native chalk.

We shall assume, in the first place, that all our readers are aware that flint is one of the manifold forms assumed by that abundant constituent of the earth's crust technically known as silica—the oxide of the element silicon. When crystallized, silica occurs in the form of rock-crystal, or quartz; but flint is one of the many non-crystalline, or amorphous, developments of the mineral. It is generally defined as a massive dark-coloured or black, semi-translucent, dull-looking variety of silica; which when pure burns to an opaque white, and has what is termed a conchoidal or shell-like fracture. The dark colour, it may be added, is due to the presence of a small quantity of organic matter, or carbon.

If we fracture a nodule of flint freshly taken from a chalk pit, we shall find that the thin edges of the sharp flakes which are seen to be produced have a pale-brown horn colour when viewed by transmitted light, and that as the flake becomes thicker the colour gradually darkens till it assumes the blue-black tint characteristic of the mass. If, however, our flake contains a portion of the external coat of the nodule, we shall see that for about a quarter of an inch in depth the outer layer is far less compact than the rest of the flint, and is of an opaque white colour. In other specimens, again, we shall observe that the colour, instead of the usual deep blue-black, is of a pale whitish blue, frequently marked by more or less distinct bands. Needless to say that on trying to scratch our flint flake with a knife we shall signally fail, and if any result happens it will be that a thin film from the metal of the blade will be left on the stone at the point of contact.

Fig. 1.—A Chipped Flint Implement, from Icklingham, half natural size. (From Sir J. Evans' "Stone Implements.")

Our flake will likewise exhibit in great perfection the characteristic conchoidal fracture of flint; that is to say, its surfaces will be smooth and undulating, swelling here into a prominent convexity, and falling there into a deeper or shallower hollow. Frequently, moreover, there may be observed a number of small parallel wavy ridges on the fractured surface. If we submit the edges of the freshly-

broken flake to a series of taps from our hammer, we shall find that a number of smaller flakes will be readily chipped off, each leaving a separate conchoidally-fractured surface on the original flake. It is this facility with which flint can be chipped, coupled with its hardness and the sharpness of its fractured edges, that induced our palæolithic ancestors to adopt it as the material for their various weapons and tools. The foregoing figure of one of these implements exhibits in great perfection the characteristic conchoidal fracture of flint.

The extreme development of this peculiar and characteristic fracture is, however, exhibited when a large flat surface of flint is struck at right angles by a round-ended hammer. The hammer then comes in contact with a minute portion of the surface of the flint, which may be represented by a small circle, and as the flint is elastic, "this small circle," as Sir John Evans observes, "is driven slightly inwards into the body of the flint, and the result is that a circular fissure is produced between that part of the flint which is condensed for the moment by the blow, and that part which is left untouched. As each particle in the small circle on which the hammer impinges may be considered to rest on more than one other particle, it is evident that the circular fissure, as it descends into the body of the flint, will have a tendency to enlarge in diameter, so that the piece of flint it includes will be of conical form, the small circle struck by the hammer forming the slightly truncated apex." A little practice will enable anyone to make these flint cones with ease. The size of the cone, and the degree of steepness of its sides, vary with the nature of the flint, the weight and form of the hammer, and the force of the blow.

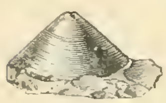


Fig. 2.—Artificial Cone of Flint. (From Sir J. Evans.)*

When examined with a lens or microscope, chalk-flint frequently exhibits a perfectly uniform structure throughout, without the least trace of the presence of any organic body. In other cases, however, traces of sponges, corals, shells, echinoderms, diatoms, &c., are more or less apparent in flint. Perhaps the most common of all these organisms are the mushroom-shaped sponges known as ventriculites; and if we examine a flint containing one of these sponges we shall frequently observe that there is a complete transition from portions of the perfectly preserved sponge to homogeneous flint, without the least trace of organic structure. In the case of echinoderms, we shall find that while in some cases the whole interior of the shell (or, as it is technically called, test) is filled with flint, the shell itself retaining its original calcareous structure, in other instances the original shell itself has been completely removed and replaced by flint. In some cases the original structure of the shell has been preserved in the flint, but more generally this has been completely lost, and the flint is structureless.

This replacement of a calcareous by a siliceous structure is an instance of pseudomorphism. Similar remarks will apply to the shells of molluscs, and likewise to corals. It is important to mention that the sponges found in flint were not originally of the horny nature of our bath-sponges, but were themselves composed of minute spicules and fibres of silica, like the so-called Venus's flower-basket of our modern seas.

With regard to the mode of occurrence of flint, we have first to mention that it is by no means confined to the chalk, but may occur in limestones of any age. In this country it is, however, more abundant and purer in the chalk than in any other formation, and may, indeed, be considered characteristic of the upper part of that formation.† Flints are found in the chalk either in the form of nodules or in thin continuous laminæ. The nodules are generally of very irregular shape, and may vary in size from a walnut to masses of a hundred-weight or so. As a rule they occur in strings at comparatively regular intervals in the chalk, generally conforming more or less closely to the original planes of bedding, and the individual nodules being sometimes at considerable distances apart, but at others closer together and more or less connected by long root-like pieces. On the other hand, the laminated or tabular flint may cut the bedding-planes of the chalk at any angle, and is often found in joints and fissures, which may emerge at the surface. As a rule, this tabular flint is devoid of organisms. Not unfrequently flints may be found which near their surfaces gradually become paler and paler in colour, and contain an increasing amount of calcareous matter, till they pass imperceptibly into hard siliceous chalk. Again, flints may be hollow, and contain in their cavities either corals or sponges, or masses of that variety of silica known as chalcedony. The latter, it may be mentioned, is a semi-transparent waxy-looking stone, generally with a more or less decided pinkish tinge, and forming mammillated or botryoidal masses. The milky and reddish varieties of chalcedony constitute carnelian, while when it is arranged in differently coloured bands it forms agate. Occasionally small crystals of quartz occur in the hollows of flint.

It has still to be mentioned that in some districts—and more especially near Norwich—in addition to the horizontal layers of nodular flints, there occur in the upper chalk a number of huge cup-shaped masses of flint placed one above another in vertical lines; these masses being locally known as "potstones," and presenting a remarkable resemblance to certain giant sponges called Neptune's cups.

The proportion of the flint to the chalk in the upper chalk of England varies, according to Prof. Prestwich, from four to six per cent. It is important to add that the masses of nodular flint may not unfrequently be found to be traversed by fractures which have subsequently been reunited; thus showing either that the substance must at the time have been in a semi-plastic condition and capable of reunion, or that the fracture has been united by the subsequent deposition of siliceous matter. It must also be mentioned that, as a rule, the white coating is confined to the outer surface of the nodules, and that we do not find layers of pure flint overlain with white coats and then again by other similar layers; thus indicating that the formation of the white coat was the final act in the development of flint.

With these observations on the structure and mode of occurrence of flint, we are in a position to enter on the more difficult subject of its origin. From the very first it was recognized by all geologists that such a peculiarly hard and homogeneous substance as flint, occurring in irregular nodules among the pure white chalk, could not have been deposited in its present condition directly from the waters of the cretaceous sea; and the problem therefore presented itself to account adequately for its mode of formation. The problem soon resolved itself first of all into two questions, namely, whether the silica was originally part and parcel of the chalk as first deposited and that it

* We are indebted to Messrs. Longman and Green for this and the figure on the previous page.

† It also occurs abundantly in the lower part of the Portland stone series of the Isle of Portland, but is there generally less pure, and has the conchoidal fracture less marked.

subsequently gained its present condition, or whether it was added at certain intervals during the deposition of the chalk from a totally different source, or was introduced subsequently to the deposition of the whole.

I believe I am right in saying that there is still a current notion among many persons who are not scientific geologists that flint is a kind of igneous rock; and its hardness and superficial resemblance to some kinds of obsidian may at first sight lend some countenance to such an idea. Nevertheless, it is almost superfluous to add, such a notion is a totally erroneous one, and the mode in which flint occurs—without altering the limestone with which it is in contact, or the fossils which it contains—is of itself sufficient to refute any such origin.

From the hardness and insolubility of flint, it would appear a very natural inference that silica in all forms is likewise insoluble, but, as proved by siliceous springs, silica in a certain condition is freely soluble in alkaline waters. This naturally suggested to the earlier geologists that flints had been deposited by the aid of hot springs or heated waters in the cretaceous sea at certain intervals during the formation of the chalk sea. Thus, the late Dr. Mantell wrote that "the nodules and veins of flints that are so abundant in the upper chalk have probably been produced by the agency of heated waters holding silex in solution, and depositing it when poured into the chalk sea." It will, however, be obvious that there are very serious difficulties in accepting any such explanation. In the first place, we should require the introduction of floods of heated water containing silica at certain irregular intervals during the whole period of the deposition of the upper chalk; and it would also be essential that these waters should have extended over vast areas of ocean. Secondly, the mode in which nodular flint occurs could not adequately be explained by any theory of this kind; while it would leave the origin of the tabular flint, which we have seen may occur in joints and fissures, totally unaccounted for. Another idea was that the silica had been, at least partially, introduced from above after the deposition of the chalk; but, although this explanation may, and perhaps does, partly account for the formation of some of the upper tabular flints, it is quite impossible that it could explain the formation of the numerous layers of nodular flints throughout the body of the chalk.

There is, however, an explanation which will readily account for all the features presented by the chalk-flints, and requires the aid of no foreign factors in the process. This explanation is based on the phenomenon known as segregation. Now segregation is the tendency presented by a small quantity of one substance, when diffused through a much larger quantity of another substance, to collect together in nodules or strings, which generally accumulate either around some fragment of their own nature or some foreign body—especially an organic one—as a nucleus. We have well-known examples of this segregating process in the huge lenticular calcareous masses termed "septaria," found in the London and Kimeridge clays, and also in the iron-nodules of other formations. Premising that soluble silica has a peculiar affinity not only for any kind of silica, but likewise for gelatinous organic substances (both of which were presented by the sponges of the cretaceous seas), it will be obvious that if we can only satisfy ourselves that at the time of its deposition the chalk contained diffused among its substance a sufficient amount of soluble silica, we shall at once be able to account for the formation of its flint. Now, as we pass upwards in the cretaceous system from the lower greensand to the upper chalk, we find a gradual change from a completely siliceous to a calcareous rock. Moreover, while in the gault, upper greensand, and lower chalk (in which in the south of England there are no flints) there is a large but decreasing amount of soluble silica, varying from 46 per cent. in the upper greensand to 31 per cent. in the chalk-marl, when we reach the upper chalk with flints such soluble silica is reduced to a mere trace. As it is at the base of the white chalk that the sponges attain their greatest development, and as it is also here that flints first commence, the disappearance of the soluble silica may be safely attributed to its segregation by means of the sponges and other bodies, and its conversion into flint. Prof. Prestwich, writing on this subject, observes that in presence of the siliceous spicules of the cretaceous sponges and their gelatinous animal matter, "the soluble or colloidal silica, dispersed through the soft chalk-mud, slowly segregated from out of the surrounding pulpy mass, and gradually replaced part or the whole of the organic matter, as it decayed away. Nor has it stopped there; owing to the affinity of the particles of colloidal silica amongst themselves, the segregation has not ceased with the replacement of the organic body, but has continued so long as any portion of silica remained in the surrounding soft matrix; whence the frequent excess of flint beyond the interior or body of the shells, echinoderms, &c., and whence also the irregular shape arising from this overgrowth of the flint nodules." Next to sponges, echinoderms seem to have afforded the most attractive centres of segregation; and while in some cases only their shells have been filled with flint, in other instances we find a mass of these shells cemented together by a nodule of flint.

In some parts of the Continent, and also in Yorkshire, we find that for some reason or other—not improbably a greater development of sponges and a smaller amount of soluble silica—the segregating process has extended downwards to the lower chalk, where flints are then found; and we suspect that in such cases analysis would also show in these beds a corresponding absence of free soluble silica.

With regard to the so-called "potstones" of the Norfolk chalk, some of which may be upwards of a yard in height, with a diameter of a foot or so, the only adequate explanation of their formation that has yet been offered is that they represent gigantic cup-like sponges which have grown one upon the top of the other, as they were successively buried in the newly-formed chalk, and that they have been subsequently silicified by the same segregating process.

We conclude, therefore, that the flints of the chalk were originally an integral portion of the rock itself, which was then a slightly silicated limestone; and that the present purely calcareous character of the chalk is due to the separation of the silica by segregation. We have, however, still to account for the relatively large amount of soluble silica present in the cretaceous rocks, since this is far in excess of what would have been brought down by most rivers of the present day, in the waters of which the amount of this substance is almost infinitesimal. It has been suggested that the unusual supply may have been afforded by the cretaceous rivers being largely fed by siliceous springs; but although this may have been one factor in the case, a more probable theory is that the drainage area supplying the cretaceous sea with sediment was largely composed of decomposed felspathic rocks, in which the amount of this silica would have been amply sufficient to have furnished the quantity present in the chalk.

It is, however, not only with regard to its mode of origin that flint is of more than ordinary interest. Being an excessively hard substance, it is one exceedingly difficult to be worn to powder by the action of water, and the

flint gravels of the valleys of the south of England, as well as the beaches of our southern coasts, and the numerous tertiary deposits composed of flint pebbles, remain to us as silent witnesses of the vast denudation of the upper chalk which has taken place in this country. Remembering that the proportion of flint to chalk is only from four to six per cent., and also bearing in mind that all the flints in our gravels have been considerably reduced in size by the action of water, we may fairly say that every cubic yard of pure flint gravel represents the removal of at the very least twenty cubic yards of pure chalk; and to this we have to add all the lower chalk which has been denuded without leaving any solid residue. Moreover, when we recollect that this denuding process has been going on ever since the eocene period, and that our river gravels only represent a small portion of the flints left by the denudation of the chalk during the latter part of this protracted period of time, we may gain some faint conception of how enormous this denudation must have been.

Although the flint gravels of our rivers afford some estimate, however faint, of the denudation of the chalk during the pleistocene period, it would be quite incorrect to assume that the flint pebbles forming the beaches of our southern coasts present a record of the amount of denudation which has taken place during the modern period. We have already mentioned that a freshly-broken flint presents a uniform blackish-blue colour throughout its interior, and any flint pebble on the seashore which had been recently derived from its native chalk would, when broken, present a similar appearance. As a matter of fact we shall find, however, that at least ninety per cent. of such pebbles are stained yellow, brown, red, or black internally, and as most of the flint fragments in many of our older gravels are likewise similarly stained, we shall have little hesitation in coming to the conclusion that our modern sea-beaches are largely derived from the breaking up of such old gravel beds, and the subsequent rounding of these irregular fragments of flint into pebbles by the action of the sea. The staining of the flints is of course due to the large amount of ferruginous matter contained in the gravels, and owing to the banded nature of the original flint it frequently gives rise to an agate-like appearance in the pebbles. Many of the pebbles in our beaches are, however, derived from still older sea-beaches, like the one now remaining at the southern extremity of the Isle of Portland, while others, again, owe their origin to the breaking up of the eocene Woolwich and Reading beds, which are largely composed of flint pebbles. Sometimes, indeed, fragments of these old beds also occur in the river gravels, where blocks of the Hertfordshire conglomerate—the equivalent of the Woolwich and Reading beds—may be met with.

We have thus abundant evidence of the exceeding indestructibility of flint, and how it may go on from one formation to another to tell, when rightly interpreted, the various steps in the denudation of our country.

In addition to being frequently stained internally, the observer will also not fail to notice that all of the flints in our river gravels have acquired a white or yellow porcelaneous external coating, quite different from the interior; and it is believed by Sir John Evans that this white coating has been produced by the removal of a portion of the flint which was still soluble "by the passage of infiltrating water through the body of the flint." That such a process must have been of inconceivable slowness, and must have required countless years for its accomplishment, goes without saying. We have, indeed, some inkling of how extremely slow this process must be, by the circumstance that the fractured surfaces of the flints built into the walls of our very oldest churches show not the slightest change from their pristine condition. When, however, we examine the chipped flint implements of our river gravels and caves, like the one shown in our first illustration, we find their surfaces altered precisely in the same manner as the flint fragments by which they are accompanied. Hence we gain, from a totally independent source, some idea of the immense antiquity of the period when the old palæolithic hunters inhabited the south of England.

Having thus reached the subject of flint implements, we feel tempted to enter into the consideration of some of their different types and the beds in which they occur, but editorial limitations of space forbid our wandering into such entrancing paths. Even, however, without entering into this part of the subject, we trust that what we have written will serve to show that a "Flake of Flint," when considered from all points of view, is to the full as interesting as a "Lump of Chalk."

THE FACE OF THE SKY FOR OCTOBER.

By Herbert Sadler, F.R.A.S.

There does not seem to be any visible diminution in the number of spots and faculæ on the solar disc. There will be an eclipse of the Sun, visible over a great part of North America, on the 20th, $\frac{91}{100}$ths of the disc being obscured. Conveniently observable minima of Algol occur at 10h. 10m. p.m. on the 14th, and 6h. 58m. p.m. on the 17th. The Nova Aurigæ has made a brief reappearance, but appears to be sinking in magnitude.

Mercury is too near the Sun to be visible in October, being in superior conjunction on the 8th. Venus is a morning star, and is still well placed for observation, although her brightness at the end of the month is only about one-half of what it was at the beginning of June, and her diameter is also decreasing notably. She rises on the 1st at 1h. 46m. a.m., with a northern declination of $12° 50\frac{1}{2}'$, and an apparent diameter of $20\frac{1}{2}''$, $\frac{56}{100}$ths of the disc being illuminated. On the 17th she rises at 2h. 19m. a.m., with a northern declination of $7° 44'$, and an apparent diameter of $18''$, $\frac{64}{100}$ths of the disc being illuminated. On the 31st she rises at 2h. 49m. a.m., with a northern declination of $2° 10'$, and an apparent diameter of $16''$, $\frac{7}{10}$ths of the disc being illuminated. During the month she passes through Leo into Virgo, being near Regulus on the 5th.

Mars is an evening star, and is better situated as to his elevation above the horizon, though his apparent diameter has markedly decreased, and, of course, his brightness also—at the end of October being only a quarter of what it was at opposition. He rises on the 1st at 4h. 10m. p.m., with a southern declination of $20° 59'$, and an apparent diameter of $16{\cdot}6''$, the defect of illumination on the following limb being very marked. On the 31st he rises at 2h. 30m. p.m., with a southern declination of $15° 13'$, and an apparent diameter of $12\frac{1}{2}''$, the gibbous form of the planet being very apparent. During the month he is in Capricornus, and makes an exceedingly near approach to the third magnitude star δ Capricorni on the 25th. Unfortunately, at the time of nearest appulse, Mars will be below the horizon in England.

Jupiter is a superb object in the evening sky, and, as we remarked in the "Face of the Sky for September," is visible to the naked eye in sunlight, the present opposition of the planet being one of the very favourable ones which occur once every twelve years. Jupiter rises on the 1st at

5h. 59m. P.M., with a northern declination of 6° 50', and an apparent equatorial diameter of 49·1". On the 12th he rises at 5h. 11m. P.M., with a northern declination of 6° 17', and an apparent equatorial diameter of 49¼". This is the day of opposition, the distance of Jupiter from the earth being 367¼ millions of miles. On the 31st he sets at 5h. 6m. A.M., with a northern declination of 5° 23', and an apparent equatorial diameter of 48½". He is occulted by the Moon on the evening of the 6th (not visible in England), and the same evening a 9½ magnitude star will be occulted by the planet, the time of central occultation being about 8h. P.M. During the month he describes a retrograde path in Pisces. The following phenomena of the satellites occur while Jupiter is more than 8° above and the Sun 8° below the horizon. On the 1st a transit ingress of the shadow of the third satellite at 9h. 47m. P.M., and a transit ingress of the third satellite at 11h. 13m. P.M. On the 3rd a transit ingress of the shadow of the second satellite at 7h. 39m. P.M., of the satellite itself at 8h. 11m. P.M.; a transit ingress of the shadow of the first satellite at 9h. 27m. P.M., of the satellite itself at 9h. 42m. P.M.; a transit egress of the shadow of the second satellite at 10h. 12m. P.M., of the satellite itself at 10h. 36m. P.M.; a transit egress of the shadow of the first satellite at 11h. 41m. P.M., of the satellite itself at 11h. 54m. P.M. On the 4th an eclipse disappearance of the first satellite at 6h. 41m. 58s. P.M., and an occultation reappearance of the same satellite at 9h. 5m. P.M. On the 10th a transit ingress of the shadow of the second satellite at 10h. 18m. P.M., and of the satellite itself at 10h. 27m. P.M.; a transit ingress of the shadow of the first satellite at 11h. 21m. P.M., and of the satellite itself four minutes later. In this case and the next transit attention should be paid to a possible projection of the satellite on the shadow. On the 11th an eclipse disappearance of the first satellite at 8h. 37m. 5s. P.M., and its reappearance from occultation at 10h. 49m. P.M. On the 12th an occultation reappearance of the second satellite at 7h. 29m. P.M.; a transit egress of the shadow of the first satellite at 8h. 3m. P.M., and of the satellite itself one minute later. On the 18th an occultation disappearance of the first satellite at 10h. 21m. P.M. On the 19th an occultation disappearance of the third satellite at 7h. 8m. P.M., of the second at 7h. 15m. P.M.; a transit ingress of the first satellite at 7h. 34m. P.M., and of its shadow at 7h. 46m. P.M.; a transit egress of the first satellite at 9h. 46m. P.M.; an eclipse reappearance of the third satellite at 9h. 53m. 46s.; a transit egress of the shadow of the first satellite at 9h. 58m., and an eclipse reappearance of the second satellite at 10h. 3m. 7s. It will be observed that for some time this evening, as on the 26th of the month, Jupiter will appear attended only by the fourth satellite. On the 20th an eclipse reappearance of the first satellite at 7h. 10m. 23s. P.M. On the 26th a transit ingress of the first satellite at 9h. 18m. P.M., of its shadow at 9h. 39m. P.M., an occultation disappearance of the second satellite at 9h. 28m. P.M., an occultation disappearance of the third satellite at 10h. 23m. P.M.; a transit egress of the first satellite at 11h. 30m. P.M., and of its shadow at 11h. 53m. P.M. On the 27th an occultation disappearance of the first satellite at 6h. 31m. P.M., and its reappearance from eclipse at 9h. 5m. 43s. P.M. On the 28th a transit egress of the first satellite at 5h. 56m. P.M., of its shadow at 6h. 22m. P.M.; a transit egress of the second satellite at 6h. 35m. P.M., and of its shadow at 7h. 26m. P.M.

Saturn does not rise on the last day of the month till 3h. 52m. A.M., and Uranus is in conjunction on the 29th. Neptune is an evening star, rising on the 1st at 8h. 0m. P.M., with a northern declination of 20° 34', and an apparent diameter of 2·6". On the 31st he rises at 6h. 2m. P.M., with a northern declination of 20° 29'. During the month he describes a short retrograde path to the N.E. of ϵ Tauri.

October is rather a favourable month for observations of shooting stars, the most marked shower being that of the Orionids, from the 17th to the 20th of the month, the radiant point being situated in 7h. 0m. R.A. and 15° north declination. The radiant point rises at the date named at about 8h. 45m. P.M., and sets shortly after 4 A.M.

The Moon is full (the Harvest Moon) at 6h. 12m. A.M. on the 6th; enters her last quarter at 9h. 37m. P.M. on the 12th; is new at 6h. 24m. P.M. on the 20th; and enters her first quarter at 9h. 26m. P.M. on the 28th. She is in perigee at 5·5h. A.M. on the 7th (distance from the earth 222,740 miles), and in apogee at 3·6h. A.M. on the 22nd (distance from the earth 252,590 miles). The greatest eastern librations take place at 0h. 7m. P.M. on the 2nd, and 8h. 8m. P.M. on the 29th, and the greatest western at 10h. 9m. P.M. on the 10th.

Chess Column.

By C. D. Locock, B.A.Oxon.

All communications for this column should be addressed to the "Chess Editor, *Knowledge Office*," and posted before the 10th of each month.

Solution of September Problem (by C. D. P. Hamilton).

1. B to Kt4, and mates next move.

Correct Solutions received from Betula, G. K. Ansell.

G. S. Cummings.—The criticism may be just, but was a little premature. Black answers 1. Q to Q4 by B to Bsq.

L. Bourne.—If, as you probably mean, 1. KB × KKtP, Black replies P to Kt8 becoming a Knight (ch), an effective resource.

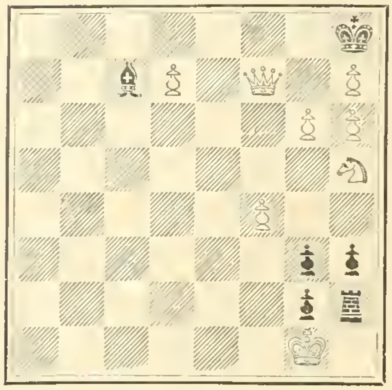
A. G. Fellows.—Thanks for the two-mover, but your key-move fails against the defence 1. . . P to B4 (or 3); whereas the problem *is* solved by 1. P to B3ch, K moves; 2. P to Q4 mate. Perhaps, however, the diagram is incorrect? You speak of 9 White pieces, of which 8 only are visible.

Betula.—Your last question is the easier to answer. Identical positions resulting from different key-moves must be regarded as practically one and the same problem. The answer to the other question must depend chiefly on (1) the number and (2) the importance of the similar variations. There can of course be no fixed rule: but assuming that a good two-mover contains four leading and more or less original variations A, B, C, D: and also about four minor variations, a, b, c, d, then a rough rule might be as follows: "If *none* of the leading variations are the same, any or all of the minor variations may occur; or if *one* of the leading variations is present (provided it is not the *chief* variation), one or two of the minor variations may occur." Thus, if the original problem is represented by A + B + C + D + a + b + c + d, I should say that E + F + G + H + a + b + c + d, if by the same composer, might be regarded as a different problem. So also C + E + F + G + a + b + e + f; *not*, however, A + E + F + G + e + f + g + h, where A is a *conspicuously leading* variation. No authority is claimed for this rule, and much, moreover, must depend on the key-move and general nature of the problem; but it seems fairly safe to consider the leading variations chiefly, neglecting the minor variations, unless of course these are greatly in excess of the others.

PROBLEM.

By G. K. Ansell.

Black.



White.

White to play, and compel Black to mate in two moves.

Appended is the third game of the recent Newcastle Match.

"Sicilian Defence."

White (E. Lasker).	Black (H. E. Bird).
1. P to K4	1. P to QB4
2. Kt to KB3	2. Kt to QB3
3. P to Q4	3. P × P
4. Kt × P	4. P to KKt3 (a)
5. Kt × Kt	5. KtP × Kt
6. Q to Q4	6. P to B3
7. Kt to B3	7. B to KKt2 (b)
8. B to BQ4	8. Kt to R3
9. Castles	9. Kt to B2
10. B to K3	10. Castles
11. Q to Q2 (c)	11. P to K3
12. KR to Qsq	12. Q to R4 (d)
13. QR to Ktsq	13. R to Ksq (e)
14. P to QKt4	14. Q to B2
15. B to B4	15. Kt to K4 (f)
16. B to QKt3	16. B to Bsq
17. P to Kt5	17. R to Ktsq
18. P to QR4	18. B to K2 (g)
19. B to Kt3	19. K to Kt2
20. P to B4	20. B to B4ch
21. K to Rsq	21. Kt to B2
22. P to B5	22. P to K4 (h)
23. B × Kt	23. K × B
24. Q to R6	24. P to Kt4
25. Q × Pch	25. K to Bsq
26. Q to R6ch	26. K to K2
27. Q to Kt7ch	27. K to Qsq
28. Q × Pch	28. B to K2
29. Q × KP	29. P to Q3
30. P to B6	30. B to Bsq
31. Q × KtP	31. Q to B2
32. B × P	32. B × B
33. R × Bch	33. K to B2
34. Q to QB5	34. R to Kt3
35. Kt to Q5ch	35. K to Kt2
36. P × Pch	Resigns.

Notes.

(a) Not good here. The move should have been made, if at all, at move 2. On his next move he would do better, perhaps, to retake with the QP.

(b) 7. . . Q to Kt3 would not be good. The White Queen would play to B4, followed by Kt to R4 sooner or later. Owing to his faulty opening, Black experiences some difficulty in castling.

(c) The Black King's Bishop being now defended, the Queen does well to retire.

(d) A favourite move of Mr. Bird's. He cannot play 12. . . P to Q4 on account of 13. P × P, P × P; 14. Kt × P!, P × Kt.; 15. B × P, as Mr. Lasker points out.

(e) Probably to make room for the Bishop at Bsq, in view of the probable posting of the White QB at B5. Perhaps, however, 13. . . P to Q4 would be preferable.

(f) 15. . . P to K4 would not only pin the Knight, but leave his Queen's Pawn still weaker.

(g) He has not much to do, but P to QR3 seems more to the purpose.

(h) This loses the game, which was still defensible by 22. . . Kt to K4.

The remainder of the game is remarkable for the rapid disappearance of the Black Pawns.

CHESS INTELLIGENCE.

A short match played at Newcastle last month, between Messrs. H. E. Bird and E. Lasker, resulted in a not unexpected victory for the latter by five games to none. There were no drawn games, though some were well contested; Mr. Lasker being generally content to exchange Queens at the earliest opportunity, relying solely on his superior skill in the end-game.

A quadrangular match was in progress at Belfast last month. Mr. Lasker abandoned his intention of competing, Messrs. Mason and Lee taking his place. The score up to September 19th was as follows: Blackburne 3, Mason 3, Bird 3, Lee 1.

The *Chess Monthly* will in future be published by Mr. Horace Cox (Bream's Buildings, E.C.). The alteration is designed as a certain cure for the attack of unpunctuality from which the leading chess magazine has recently been suffering.

A match between Dr. Smith and Mr. T. Block, played recently at the City of London Chess Club, resulted, after a very hard fight, in a victory for Dr. Smith by five games to four.

Contents of No. 83.

TERMS OF SUBSCRIPTION.

"Knowledge" as a Monthly Magazine cannot be registered as a Newspaper for transmission abroad. The Terms of Subscription per annum are therefore as follows:—To any address in the United Kingdom, the Continent, Canada, United States, Egypt, India, and other places in the Postal Union, the Subscription is 6 shillings, including postage; or 1 dollar 50 cents; or 6 marks; or 7 francs 50 cents.

For all places outside the Postal Union, 5 shillings in addition to the postage.

Communications for the Editor and Books for Review should be addressed, Editor, "Knowledge" Office, 326, High Holborn, W.C.

KNOWLEDGE

AN ILLUSTRATED
MAGAZINE OF SCIENCE
SIMPLY WORDED—EXACTLY DESCRIBED

LONDON: NOVEMBER 1, 1892.

CONTENTS.

THE DISASTER AT ST. GERVAIS.

By the Right Hon. Sir Edward Fry, LL.D., F.R.S.

WHILST at Chamonix this summer I was much interested in obtaining the best information which I could as to the physical causes of the destruction of the baths of St. Gervais, which excited such a widespread interest and sympathy. I had the advantage of studying with M. J. Tairraz, photographer of Chamonix, the photographs which he had taken the day after the event, and of conversing with M. Venance Payot, who was also on the spot about the same time, and with one or two other persons who visited the site somewhat later. From this information, and from what I myself saw, I was able to come to a pretty clear view of what had happened. Since my return to England I have studied the very interesting paper on this subject by M. Vallot, the director of the Mont Blanc Observatory (who visited the spot on the 19th July), which appeared in *La Nature* of the 20th August, as well as the note signed "D. W. F." in the "Proceedings of the Royal Geographical Society" for August, and the numbers of the *Times* newspaper and of *Nature* which contained information or correspondence on the subject.

In the hope that what has so much interested myself may interest some of the readers of Knowledge, I have written this paper, with the view of endeavouring to give some account of the causes of the catastrophe, not of giving any detailed account of the event itself. Fig. 1 shows the baths of St. Gervais before the disaster; Fig. 2 shows their remains after it. A comparison of these two views will sufficiently bring to the mind the nature of the destruction caused by the event. These views, like all the others which illustrate this paper, are after photographs taken by M. Tairraz.

The accompanying sketch map (Fig. 3) will enable the reader to follow the narration of this terrible disaster. The western slopes of Mont Blanc (the summit of which lies beyond the south-east corner of the map) descend into the valley of the stream known as the Bon Nant, which, rising in the Col du Bonhomme, runs nearly due north. The well-known heights of the Dôme du Gouter and the Aiguille du Gouter give off glaciers which, through tributary valleys, send their waters into the Bon Nant, and amongst these the glacier de Bionnassay. Above this glacier to the north are found glaciers called the glacier de la Gria and the glacier des Têtes Rousses. The stream from the Bionnassay glacier will be seen to flow below the village of Bionnassay, and close to the village of Bionnay to join the stream of the Bon Nant. This continues to flow northward till about half-a-mile above St. Gervais it enters a narrow and deep gorge. Just where this gorge opens out, the baths of St. Gervais were built across the valley—there about 100 feet in width. From the baths, the Bon Nant continues its course northward till it joins the Arve, near the village of Le Fayet. The gradients of this piece of country are steep. The Aiguille du Gouter is 12,710 feet high above the sea-level, the Têtes Rousses about 9000 feet, the baths 2060 feet, and Le Fayet 1950 feet. Such is the general character of the locality.

Early in the morning of Tuesday the 12th July last, a terrible volume of water, ice and mud overwhelmed the baths and destroyed many lives.

The source of this calamity is to be found high up in the glacier des Têtes Rousses.

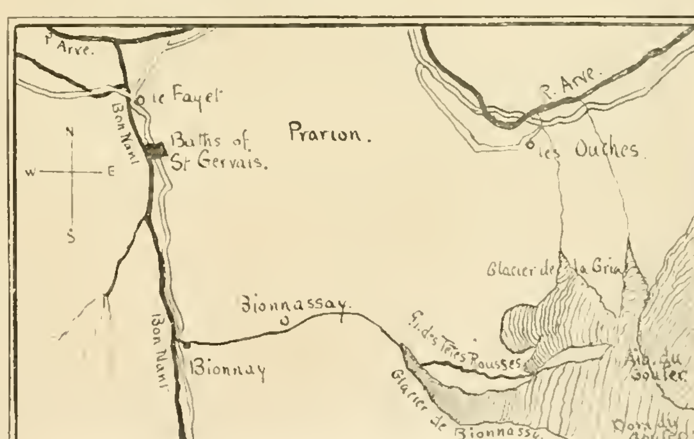


Fig. 3.—Sketch Map of district surrounding St. Gervais.

The enlarged sketch map (Fig. 4) will enable my readers to understand the locality more exactly. The large glacier marked A is the glacier de Bionnassay; the glacier immediately to the north of it, and marked B, is the glacier des Têtes Rousses; C is the glacier de la Gria. The upper part of the glacier des Têtes Rousses is nearly a plateau, and is confined, to the south, by rocks which divide it from the glacier de Bionnassay, and partially to the west by the rocks of Têtes Rousses, E F.

Between E and F the glacier des Têtes Rousses falls rapidly by a steep incline of something like 45°.

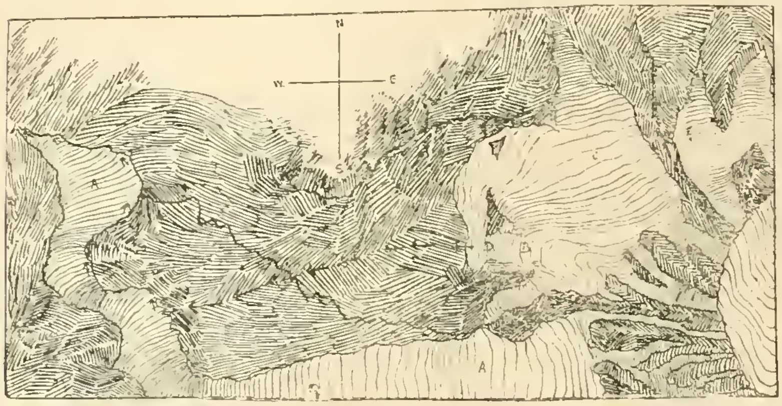


Fig. 4.—Sketch Map of Glacier des Têtes Rousses and Surroundings. A, Glacier de Bionnassay; B, Glacier des Têtes Rousses; C, Glacier de la Gria; D, lower cavity; E, F, Rocks of the Têtes Rousses.

On the plateau of the Têtes Rousses glacier, near where the letter B appears on the plan, there was formed in the glacier a great cavity filled with water, a glacial lake: it was probably entirely sub-glacial, *i.e.*, no part of the water was visible on the surface. Whether anyone had passed over or near this part of the glacier this year, before the catastrophe, seems doubtful, but last year no one had noticed the existence there of any lake. This cavity was about 260 feet by 130 feet in superficial extent, and had a vertical depth variously estimated from 130 to 160 feet. This cavity communicated by a passage through the ice with another cavity situated in the sharply inclined part of the glacier, and at or near the spot marked D on the map (Fig. 4).

Neither of the cavities disclosed the rocky floor of the glacier; the floor both of the passage and of the upper cavity was formed of broken masses of ice which had fallen in. Whether the solid ice of the glacier or the rock lay immediately beneath these broken bits was not apparent. M. Tairraz believed the entire system of cavities to be in the substance of the ice.

M. Vallot describes the lower hole as having a diameter of 40 metres, or about 130 feet, and a depth of 20 metres, or about 65 feet. M. Tairraz described it as much shallower. M. Vallot describes the passage as having a length a little over 50 metres, or from 160 to 170 feet, whilst M. Tairraz estimated its length at nearly twice as much.

M. Tairraz, who visited the spot the day after the accident, and M. Vallot and his party, who visited it on the 19th of July, all passed from one hole to the other through the passage, though not without difficulty from the masses of ice which had fallen in from the roof.

Fig. 5 shows the upper of the two cavities. It will be observed that the roof fell in without any disturbance of the surrounding glacier.

Fig. 6 shows the lower of the two cavities, the cavity on the incline. From this cavity ramifying passages were observed by M. Vallot, and may have led to other reservoirs of water. Near this cavity the glacier has been strangely changed by the event. The ancient level of the glacier was continuous with the upper line on the horizon; the event has destroyed this continuity, and has set free a vast mass of ice which has slidden away and disappeared, leaving behind the great ice cliff which appears in the drawing, which is some 130 feet in height, and which stretches in a crescent form the whole way across the glacier between the points E and F on our last map. Fresh snow fell almost immediately after the event, and is seen on the lower part of the picture. It wiped out all traces of the disruption on the new surface of the glacier. But both in the cavity itself and on the newly-formed ice cliffs are apparent the lines which indicate the divisions between the layers of ice formed in successive years.

Towards the bottom of the hole will be observed a cave-like hollow, which is the commencement of the sub-glacial passage to the upper cavity.

The imaginary section which forms Fig. 7 will perhaps help further to explain the situation. A is the upper cavity with its roof yet intact, B is the sub-glacial passage, and C the lower cavity. The dotted line will indicate and bound the mass of ice which was detached by the accident. The horizontal lines indicate water, and the oblique lines rock.

What seems to have happened was this: that on the morning in question the roof of the upper cavity fell in with a momentum which was sufficient to drive the water violently through the passage and into the lower cavity. In this cavity the upward pressure of the water was sufficient to detach the superincumbent mass of ice and to set it rolling rapidly down the incline. The roof of the upper cavity must have operated like the plunger of a piston and produced a sudden and intense pressure throughout the whole water cavities. The ice, being less thick above the second cavity than above the passage, yielded, and in its turn operated by suction on the water in the cavity and drew it after it. I suppose that the sudden slipping away of a great mass of ice would tend to produce a vacuum, which operating on the lower hole drew

Fig. 7.—Imaginary section of the two cavities, with connecting passage. A, upper cavity; B, passage; C, lower cavity.

the whole contents out of it and of the adjoining cavities. In this way only can I account for the vast mass of water which formed part of the descending mass, and for the fact that on the day after the accident the two cavities appear to have been empty of water. It seems to me probable, as I shall hereafter mention, that the ice above the lower cavity was more or less loosened by crevasses running across the line of motion of the glacier and therefore in the same line as the cliff left by the departed mass of glacier.

The mass of ice and water thus set free slid over the steep glacier or nevé to the west, and then turned in a

Fig. 6.—The Lower Cavity in the Tetes Rousses Glacier.

Fig. 5.—Plateau of the Tetes Rousses Glacier, showing the Upper Cavity, the roof of which fell in on the morning of Tuesday, the 12th of July, 1892, causing the Disaster at St. Gervais.

south-westerly direction down a fold of the hills which crosses from the glaciers in question to the glacier de Bionnassay; when it reached the side of this glacier it impinged on the lateral moraine, but was driven back by this against an older moraine which it scooped out into a great cirque. These moraines yielded to the impetus of the moving mass of water and ice, and joined in the mad race which the rushing mass now pursued down the valley between the older lateral moraine and the present lateral moraine, until it reached the bottom of the Bionnassay glacier, near which it joined the stream from that glacier, and thenceforward travelled in its course, overturning first a châlet in the wood, where were an old woman and some cows: then leaving the village of Bionnassay safe on its height above the stream it passed downward to Bionnay, which it almost entirely destroyed, joined the course of the Bon Nant and rose as the gorge narrowed to a great height, from which it fell with remorseless energy on the doomed baths of St. Gervais. Thence it continued its course through the beautiful grounds of the establishment till it reached the junction of the Bon Nant and the Arve, at Le Fayet, where it spread out into a horrid flood, that is now represented by a great expanse of grey mud and sand and wreckage, extending far and wide over what was before a populous and fertile stretch of country.

The course of the avalanche as above described is traced on the second sketch map by arrows which indicate its direction. It may in like manner be traced on Figs. 8 and 9. Fig. 8 is a general view of this part of the mountain range taken from Prarion, north-west of the glacier des Tetês Rousses; and Fig. 9 is a key to Fig. 8, indicating the course of the torrent by arrows.

Some notion of the momentum of the moving mass of ice, mud, and water may be gained not only from the destruction of the buildings at the baths of St. Gervais, but from the movement of great stones, or rather rocks, which it effected. One in particular, of vast dimensions, is shown on the right hand of Fig. 10. The villagers of Bionnay were intending to celebrate a *fête* on the 14th July, and with a view to letting off *feux de joie* on that occasion holes had been bored in a stone then in the village. That stone, with its holes, is now at St. Gervais, and was probably highly effective in the destruction of the baths. It is rather a rock than a stone. It is further stated that the iron safe in the office of the baths was carried five miles down the stream to Sallanches, where it was found (*Times*, 15th July, 1892). These facts are of great interest, as the power of water to move great masses of stone for long distances has been a moot point amongst geologists and students of glaciers.

This explanation leaves many things unexplained. Under what circumstances will water accumulate as water in the body of a glacier? What, in this particular case, caused the two great cavities and the communicating passage? Were they formed by some blocking up of "moulins," or "glacier wells," as suggested by Mr. Justice Wills in his letter to the *Times* (16th July, 1892), or "originally caused by sub-glacial collapse" as suggested by Mr. Von Lindenfeld in *Nature* (15th September, 1892)? [See also the letter of M. Delebecque in *Nature* of the 22nd September, 1892.] How did the falling roof of the upper hole acquire sufficient momentum to do all the work that it did? Was its weight augmented by some mass of snow or ice that fell suddenly upon it from above? Was the water in the cavities adequate to carry down the vast mass of matter which descended? or was it increased from water stored in crevasses which have disappeared with the fall of the glacier? or was ice reduced into water by friction in the downward rush of the mass (as Prof. Forel would have us believe)? Was the impact of the water the sole cause of the detachment of the glacial mass, or was there some rent in the glacier existing before the water operated which predisposed the glacier to break up under any blow which it might receive? Was the draught produced by the sudden slip of the glacier sufficient to evacuate the two holes of their fluid contents, or was there some other co-operating cause? Lastly, is the account above given in error in suggesting that the roof of the upper cavity was the first thing to move? or was it, on the contrary, the glacier on the slope which gave the initial motion, and did the roof of the upper cavity only yield to the suction produced by the sliding ice? These questions I can ask, but I cannot answer.

Fig. 9.—v Aiguille du Gouter. v v Aiguille de Bionnassay. v v v Glacier de Bionnassay. The arrows indicate the course of the torrent.

I have suggested that it is probable that the slip of ice was due to two causes—the one, an accumulation of snow and ice, so great as to cause a fracture on the slope, and the other, the action of the water on this unstable mass.

As regards the first of these causes, the facts in reference to the glaciers of Mont Blanc are in favour of it. It is well known that for some years past these glaciers have been on the increase, that the balance of deposition and waste is now in favour of deposition, and not as it was

for many years before in favour of waste. The glacier of Argentieres has been observed to have increased during the last few years in length, height, and width. The Mer de Glace is recovering some of its lost ground and, one may add, some of its lost beauty; the glacier de Bossons is advancing and overturning many of the sapling spruces which, in the period of its retreat, have imprudently ventured to grow on its moraines; and the like phenomena have been observed in other parts of the Alps.

As regards the outburst of water from the glaciers, it may be worth while to mention an event of the same kind, though on a much smaller scale, which has since occurred on another flank of Mont Blanc. On the eastern side of the glacier de Bossons, high up and very nearly under the Pierre Pointue, a stream flows from the glacier and rushes down the mountain side in a wide channel, of which in ordinary times it occupies only a small part. About ten o'clock in the morning of the 11th August a sudden outburst of water took place at this spot, and rushed as a muddy torrent down the water-course, carrying with it great stones, masses of ice and trees, destroying the bridge which crossed the stream, and at one spot breaking over the high bed of the stream and finding a new course for itself through the forest. At the time I was going up the Brevent and I heard the great roar of the torrent, and some of my party saw it descend. The next day I visited the spot where it had flowed over the bank, and found the forest there strewn with trees, torn up or snapped off, with blocks of stones of cubic dimensions to be measured by feet or yards, with blocks of ice and a layer of mud and sand. Many trees which had withstood the onset of the flood had been stripped of every twig, every leaf, and every particle of bark. On the trees which stood on the high bank above the stream, the flood had left a coating of mud as high as I could reach.

Both the St. Gervais outbreak of water and this on the glacier de Bossons occurred before the greatest heat of the summer that has just passed had set in.

It has been suggested that a part of the water which came down upon St. Gervais may have been due to the mass of ice forming a dam and so accumulating the water of a stream till it rose to such a volume as to burst through the dam. But the facts, so far as I can gather them, do not favour this suggestion. No one appears to have seen such a dam, or to have seen any trace of it after the event. No stream would seem likely to have been so dammed except that from the Bionnassay glacier. There seems to be no doubt but that the châlet near the foot of the glacier was destroyed on the same night as the baths of St. Gervais.

I am indebted to Monsieur J. Tairraz for permission to use, for the purpose of the engravings which illustrate this paper, some of the very interesting series of photographs illustrative of the event which were taken by him, and which are well worth the attention of all who are interested in such matters. I am further indebted to him for a great deal of information most freely and clearly communicated both by word of mouth and by letter.

CATERPILLARS.—I.

By E. A. Butler.

By their frequently attractive colours, the ease with which most of them may be reared, the startling nature of the changes they undergo, and the great beauty of the resulting perfect insects, the larvæ of Lepidoptera have long since made themselves general favourites. Childhood's first essays in the direction of practical natural history often consist, to the more or less serious detriment of the comfort of the household, and the neglect of nursery proprieties, of the pastime of "keeping caterpillars," while the zealous work of manhood's maturer years may be given, as in the case of the late Mr. Buckler, to the delineation of their forms and the unravelling of their life-history. They sometimes force themselves unpleasantly on the notice of the community by the violence they do to agricultural interests or to æsthetic sensibilities, when they multiply to such an extent as to cause either great destruction or at least unsightly disfigurement of cherished objects of culture in the field or garden. They have sometimes proved so great a pest as to have earned for themselves a place in the records of history, and their name has become proverbial as that of a national scourge. In the days of mediæval superstition, when the animal creation was supposed to have its duties and responsibilities to humanity as well as humanity to it, caterpillars, along with other destructive creatures, were made the subjects of lawsuits and cited as defendants in the civil and ecclesiastical courts, where the penalties of the law were solemnly pronounced against them, and the curses of the Church and the terrors of excommunication held over them if they did not leave the district within a specified time. At such trials the accused were not always represented by proxy, but were sometimes caused to appear in their own persons before the judges; thus in 1451, during a plague of leeches in Switzerland, the Bishop of Lausanne suggested the advisability of procuring some of the aquatic worms and placing them before the magistrates. This was done, and they were ordered to leave the district in three days on pain of falling under the ban of the Church! In more modern days, on the other hand, caterpillars have become the handmaids of science by reason of their ready adaptability to observation and experiment; not only has their anatomy been duly investigated, but in connection with their peculiarities of form, colour, and markings, they have formed, and are still forming, the subjects of researches which may be expected to throw much light on several fascinating biological questions. If it be asked, for instance, why caterpillars are coloured and shaped as they are, it is not nowadays considered a sufficient reply to say that Nature is prodigal of beauty, but even the minutest peculiarities of marking and form are eagerly scrutinized and compared, in the hope of discovering facts bearing upon the pedigree of the creatures, or the influence of their environment upon them.

A caterpillar's life is not a very eventful one. The daily programme is rather monotonous, consisting of alternations of eating and ceasing to eat, and as a provident mother has usually placed it in such a position that it is from its earliest hours surrounded with abundance of food of the proper sort, there is little call for the exercise of any superior intelligence in satisfying the somewhat imperious demands of the periodically recurring hunger. But uneventful though on the whole it is, there are moments of excitement which cause its long thin-walled heart to beat more rapidly beneath its back, and its watery-looking blood to course through its body with greater vigour than usual. There is first, for example, the act of hatching. The walls of the often prettily ornamented little eggshell are nibbled through, and a big-headed but otherwise rather worm-like creature issues from the opening, prepared to make an immediate onslaught on the provisions in its neighbourhood. The amount of eggshell eaten in accomplishing this deliverance varies in different cases, the caterpillar being sometimes satisfied with making a hole just big enough to escape through; but, on the other hand,

Fig 10.—Great Boulder moved by the Torrent from the Village of Bionnassay.

Fig. 1.—The Baths of St. Gervais before the Disaster.

Fig. 8.—The Aiguille du Gouter and the Glacier de Bionnassay, down which the Torrent rushed.

Fig. 2. The Baths of St. Gervais after the Disaster.

sometimes the entire shell is devoured at the first meal. Almost the only regularly recurring excitement is that of changing the skin, a process which may take place some five or six times, the exact number depending upon the species. It appears to be frequently an operation of considerable trouble and difficulty, and is preceded on each occasion by a short period of rest and abstinence. These moults are not equally distributed through the life of the caterpillar, and the longest interval is that between the last moult as a larva and that which produces the change to the pupa state.

The danger of attack by insectivorous birds and reptiles, by parasitic insects such as ichneumon flies belonging to the families *Ichneumonidæ* or *Braconidæ*, or dipterous flies belonging to the *Tachinidæ*, or by rapacious ones such as the fossorial Hymenoptera, many of which provision their nests with small caterpillars, are features in the life of those larvæ that feed in the open, which, together with the disturbing effect of storms, threatening to shake them from their perches, must give a little zest, if the creatures are only able to appreciate it, to an otherwise flat and tame existence. But in order to guard against such dangers, there is very little in the way of active resistance; indeed, the insects are so delicately constructed that it is not safe for them to struggle or fight; the soft-skinned body is kept in a tense condition by the fluid it contains, which is therefore under pressure, so that any little wound would occasion a considerable loss of blood, which would have the double effect of producing the enfeeblement that always follows blood-letting, and lessening the creature's control over its movements by making it flaccid. Hence a little damage to the skin might easily prove fatal, and therefore the caterpillar cannot afford to be very pugnacious, or to defend itself strenuously if attacked. Therefore the means used are mainly of the passive kind, such as are supplied by protective coloration, a clothing of hairs or spines, or the assumption of a particular attitude, and if these fail, the caterpillar has but to submit to its fate.

In the articles on ants, published in Knowledge some months ago, it was pointed out that some tropical species of ants assume a sort of guardianship over certain caterpillars for the sake of a secretion they yield, which is palatable to their guardians. A very curious instance of this has been recorded by a reliable American entomologist, the insect protected being the caterpillar of one of the "Blues," called *Lycæna pseudargiolus*. The larva was seen on its food-plant, and "on its back, facing towards the tail of the larva, stood motionless one of the larger ants. . . . At less than two inches behind the larva, on the stem, was a large ichneumon fly, watching its chance to thrust its ovipositor into the larva. I bent down the stem," says the observer, "and held it horizontally before me, without alarming either of the parties. The fly crawled a little nearer and rested, and again nearer, the ant making no sign. At length, after several advances, the fly turned its abdomen under and forward, thrust out its ovipositor, and strained itself to the utmost to reach its prey. The sting was just about to touch the extreme end of the larva, when the ant made a dash at the fly, which flew away, and, so long as I watched, did not return." The disturbance created by the ant's action apparently caused the caterpillar for the first time to realize its danger, and it immediately began to lash its fore parts from side to side, thus bringing into requisition its only means of defence.

Many caterpillars are internal feeders, and live within the trunks of trees, or the stems and roots of reeds, grasses, and other herbaceous plants, in the interior of fruits, or in mines between the cuticles of leaves. Here they are, of course, more out of the reach of their foes; lizards cannot get at them at all, while insectivorous birds find it much harder work to reach them, and there is small chance for the ichneumon flies, except for such as are provided with a long ovipositor to be thrust into the burrows. Hence the life of such caterpillars must be far more monotonous and uneventful than that of outside feeders. In particular, their locomotion is much restricted; they have to tunnel wherever they go, and sometimes, as in the case of leaf-miners, the whole area of their lifelong wanderings does not amount to more than a small fraction of a square inch. Such caterpillars do not possess the special means of protection by which their external-feeding relatives are distinguished; they are usually naked and whitish, and without adornment, and the walls of their prison-like home are a sufficient safeguard. It is curious that for the clearest indications of intelligence during larval life we must go to the smallest species; amongst the leaf-rollers, or Tortrices, some of which, such as the green oak moth (*Tortrix viridana*), completely strip the trees of their young leaves in early spring, and the Tineæ, the minutest of all Lepidoptera, we find some remarkable instances of constructive power, as shown in the making of shelters out of rolled-up leaves, or in the cutting out and piecing together of cases for the protection of the hinder part of the body. Some caterpillars, again, are social in habits, constructing a common abode in the form of a web.

A caterpillar has been called a "locomotive egg," in allusion to the primitive condition of its organization as compared with that of the adult insect. This primitive condition is shown partly in the entire absence of several structural details—such, for example, as wings, which the perfect insect is possessed of—and partly in the more rudimentary and less concentrated and specialized form of those that it possesses in common with the adult, when compared with what they will ultimately become; this characteristic is well shown in the condition of the nervous and reproductive systems. With regard to the former of these, there is in the adult a fusion of parts which are separate in the larva, the number of ganglia in the nervous chain being greatly reduced, while the nerve centres of the head and thorax are much increased in size. With regard to the latter, caterpillars are not sexually mature, and are therefore functionally neither male nor female; but the reproductive organs are actually present, though in a rudimentary condition, so that usually no difference is discernible externally between those that will produce male insects and those that will produce females. This is not, however, always the case; sometimes certain portions of the reproductive organs become sufficiently distinct to be able to be seen through the skin, and the separation of the sexes can thus be made; or again, there is a difference in ornamentation which is dependent upon sex—for example, the caterpillars of the vapourer moth (*Orgyia antiqua*), a common insect often seen even in the streets of large towns, are of very different sizes, the small ones producing males and the larger females, while they differ also in the colour of the tufts of hairs with which they are so fantastically adorned. From these details it will be seen that in caterpillars a condition of things is exhibited which would in some other animals be a merely transitory stage in their early embryonic history. On the other hand, the larval stage, which is not unfrequently the longest division of the insect's life, embraces the whole period of growth, and in fact the insect's actual bulk at the end of its caterpillar career is generally greater than during its subsequent history, the change into the chrysalis being attended with a diminution in size, while, in like manner, during that period, there is a gradual loss of weight, due

largely to evaporation, so that the perfect insect is, as a rule, both smaller and lighter than the caterpillar which produced it. After caterpillar days are over there is no further growth in the sense of mere increase in bulk, but only in the sense of development, *i.e.*, gradual, though it looks like sudden, change of form of already existing organs, or the acquisition of new ones at the expense of matter already present in the organism.

Let us look now at the general structure of a caterpillar. Whatever be the peculiarities of its actual outline, its more or less cylindrical and worm-like frame is obviously divisible into two very unequal parts, head and body, the latter being again subdivided by constrictions at regular intervals into twelve segments. The head (Fig. 1) is covered with a hard skin, the greater part of the area of which is divided into two equal lobes (*a*), one on each side; from their position and their often highly-polished appearance they are rather suggestive of eyes, and are sometimes supposed to be such, an idea which is quite erroneous; the true eyes are twelve in number, six minute glassy knobs (Fig. 2) being placed on one side and six on the other, not far from the mouth apparatus. A lens is needed to see them properly, even in a large-sized caterpillar, and from their low position and their minute size they can hardly be of any great use to the insect; indeed, the groping movements of its fore parts, when it is not actually on its food-plant, seem to indicate a purblind condition, so that it probably conducts its movements as much by the sense of touch as by that of sight. That caterpillars are, however, sensitive to light to some extent has been shown in various ways. For example, the caterpillars of the small tortoiseshell butterfly (*Vanessa urticæ*), when kept in a glass cylinder before a window, have been found to take up their quarters on the side nearest the light, and when the cylinder was turned round so as to bring them over to the other side, they quickly shifted their position and assembled again in the sunlight. It is to be remembered that this species, when on its food-plant, the common stinging nettle, delights to cling round the topmost parts in the full glare of the sunshine, so that in confinement it was but endeavouring to follow out its own natural instincts. On the other hand, such species as feed in a more or less retired position would, no doubt, under similar circumstances, have sought the darkest corners of their prisons. Plateau has conducted experiments with the view of determining the extent of the visual powers of caterpillars and other insects. As a result of these experiments, he has been led to believe that the eyes of caterpillars are able to do more than merely distinguish light from darkness, and that the insects really do see, though but poorly. They are, in fact, very near-sighted, their distance of distinct vision being no more than about a centimetre, that is, under half an inch, quite far enough, however, to enable them to see to eat their food. At greater distances they can perceive large masses, but do not seem to be able to make out their nature. Moving bodies also do not affect them except within the range of distinct vision. Caterpillars seem to have a certain sense of direction; at any rate, they have a tendency to crawl *upward*, even when the route taken does not lead to their food-plant, a circumstance which appears to imply a recognition of the fact and direction of gravitation.

Fig. 1.—Head of Buff-Tip Caterpillar (*Pygæra bucephala*), with jaws open: *a*, lobes of face; *b*, clypeus; *c*, part of labium; *d*, labrum; *e*, mandibles; *f*, antennæ.

Fig. 2.—Cluster of ocelli of Silkworm Caterpillar; right side.

In the angle between the receding edges of the above-mentioned lobes is situated the *clypeus* (Fig. 1, *b*), a triangular area which forms the upper boundary of the feeding apparatus. On its lower edge abuts a movable lid, the *labrum* (*d*), or upper lip, which guards the entrance to the mouth above. Immediately below this is the most important part of the whole apparatus, a pair of exceedingly stout jaws, or *mandibles* (*e*), which are placed at the sides of the mouth, and when closed meet in front of its aperture. The lower border of the mouth is constituted by the *labium* (*c*), or lower lip, a complex structure formed by the coalescence of many parts. There is a central portion, which is the *labium* proper, with a pair of minute appendages, its *palpi*. The two side parts are the *maxillæ*, and these also carry palpi. On the central portion is the minute perforation which forms the opening of the ducts from the silk glands, whence issues, in the form of a thread, the gummy substance that is used for such a variety of purposes in different species, as for example, forming the cocoon or the attachment of the chrysalis to some support, binding leaves together to form a shelter, or creating a common dwelling-place in the form of a thick web, or serving as a means of anchorage and a ladder for return when a fall from the food-plant takes place. Finally, at the outermost edge of the mandibles are placed a pair of small jointed organs, similar to the palpi in structure, and capable of "telescoping up"; these are the antennæ (Fig. 1, *f*), which are much used in testing the path and surrounding objects. Close by the antennæ are situated the eyes, or rather ocelli. To verify these points it is well to secure as large a caterpillar as possible, since at the best of times none of the structures referred to are other than small. Insects such as the larva of the goat moth (*Cossus ligniperda*) which formed the subject of Lyonet's anatomizing a century ago, or the privet hawk moth (*Sphinx ligustri*) which served a similar purpose to Newport half a century later, answer very well; or in default of these, large specimens of that extremely abundant creature, the larva of the buff-tip (*Pygæra bucephala*), which in our own times furnished Sir John Lubbock with material for a careful study of caterpillar muscles, may be used.

(*To be continued.*)

THE REV. JOHN MICHELL, ASTRONOMER AND GEOLOGIST.

By John Richard Sutton, B.A.Cantab.

(*Continued from page* 191.)

At forty-two years of age Michell settled down as Rector of Thornhill, Yorks, a small parish on the River Calder, between Huddersfield and Wakefield, memorable for its connection with the heroic royalism of the Saviles,* to whom the gift of the living belonged. Michell seems to have made the acquaintance of a member of this family at Cambridge, for a certain George Savile, of Queen's College (who was afterwards "Baronettus," and probably, therefore, the son and heir of Sir George Savile, of Thornhill), took the M.A. and LL.D degrees together in 1749†

* Whitaker's *Loidis and Elmete.* † *Cantabrigienses Graduati.*

(the year after Michell got his fellowship), chiefly, it would seem, in recognition of his wealth and prospects. It is a curious exemplification of the tenacity with which feudal instincts clung to the University in those comparatively recent days, that whereas "Philip, Duke of Newcastle, John, Earl of Sandwich, and George Savile, Esquire," *graced* all the public ceremonies of the University, the "Masters and Fellows in their robes" merely *attended*.

Mr. Ansted, who, by the way, knew more than most people about Michell, remarks that Michell "appears to have discontinued scientific pursuits on succeeding to the living; at all events," he adds, "nothing more was made public by him during the remainder of his life."* This is quite wrong, though, seventeen years afterwards, namely, in 1784, Michell published in the *Philosophical Transactions* an ably-reasoned article "on the means of discovering the distance, magnitude, &c., of the fixed stars, in consequence of the diminution in velocity of their light, in case such a diminution should be found to take place in any of them, &c." This discussion exhibits Michell's marvellous talents unimpaired; but it will not be necessary to consider it here, for it was founded on Newton's erroneous corpuscular theory of light, then generally accepted. Had that theory been tenable, Michell's subtle suggestions might have been applied with success in the course of time. As it was, when the corpuscles had succumbed to Young's vigorous assault, a modification of Michell's plans was adopted to find the velocities with which some of the stars are moving from or towards the earth, albeit nothing is learned in this way of their distances. One result worked out in this paper of 1784 is too pretty to be passed over. If, said he, light be sent out in the form of material particles from luminous bodies (as Newton supposed), these particles must be amenable to gravity, hence their original velocities would be considerably modified by the retarding action of the emitting bodies; and in cases where these last were large enough, the corpuscles would be forced to return to their starting places. The consequence would be that the larger stellar masses would be totally invisible. Advanced knowledge has taught us that this result is not in accordance with facts, and that the mass of the luminous body can have no effect on the velocity of the light it sends forth. Yet arriving, strangely enough, at some truth from erroneous premises, Michell managed to deduce the result, whose observational proof was not forthcoming for the best part of the following century, that by the motions of some of the bright stars we might be able to detect the existence of dark companions to them; and this, when astronomers as yet did not recognize the existence even of lucid binary systems.

In the seclusion of Thornhill, Michell thought out *and constructed* the celebrated torsion balance for weighing the earth. Doubtless it was the outcome of his magnetic experiments, not that he confounded magnetism with gravitation. It is still the best instrument we have for the purpose, and the measure of the earth's density obtained by this very instrument (somewhat modified, it is true, in minor details) gave the first measure of the earth's density. An explanation of the machine would be out of place here, but a complete account will be found in the *Philosophical Transactions* for 1789 by Michell's friend Henry Cavendish, who first used it, Michell having died too soon to use it himself.† There is also a simple account in Young's *General Astronomy*, and by the late Mr. Proctor in the early part of the *Old and New Astronomy*. Archimedes is reported to have said that, given a lever long enough, he would move the earth. Of Michell we may say that he showed how to put the earth in the balance and weigh it. In this, as in other things, Michell has been undervalued. He seldom gets the credit for his invention; more frequently than not it is stated that Cavendish himself designed the machine. Fortunately, Cavendish's own declaration to the Royal Society is explicit enough: "Many years ago," he says, "the late Rev. John Michell, of this Society, contrived a method of determining the density of the earth, by rendering sensible the attractions of small quantities of matter; but as he was engaged in other pursuits, he did not complete the apparatus till a short time before his death, and did not live to make any experiments with it. After his death the apparatus came to the Rev. Francis John Hyde Wollaston, Jacksonian Professor at Cambridge, who, not having convenience for making experiments with it in the manner he could wish, was so good as to give it to me." Nothing could possibly be clearer, and it may well seem surprising, in the face of such a statement, that misapprehension should afterwards arise. That it did arise is probably due to Lord Brougham, whose article on Cavendish was widely read.‡ Unfortunately, the well-merited drubbing he got from the *Quarterly Review*§ was not taken to heart by later writers as it should have been. Lord Brougham admitted subsequently that he had not so much as seen Cavendish's paper on the torsion balance before publishing his sentiments concerning it. But whereas many persons read the article wherein the invention of the torsion balance is assigned in large type to Cavendish, it is to be feared that few read the recantation in small type in an obscure corner of a later volume.

After a life of quiet usefulness, which will be more and more appreciated as time goes on, Michell passed away on the 21st of April, 1793. He was buried in the south chancel of St. Michael's, Thornhill. A flat plain stone marks the spot, and another by its side is in memory of his wife. The inscriptions are in plain capitals:—

(1) "Rev. John Michell,
Died the 21st April, 1793,
Aged 68 Years."

(2) "Anne Michell,
Relict of the Rev. John Michell,
Died the 3rd November, 1818,
Aged 86."

A nobly-worded but curiously punctuated tablet hangs in the church to Michell's memory, and to that of his brother. It runs thus:—

"In the chancel of this church are deposited the remains of the Revd. Jno. Michell, B.D., F.R.S., and 26 years Rector of this parish, Eminently distinguished as the Philosopher, the Scholar. He had a just claim to the character of the good Christian. In the relative and social duties of Life; the tender Husband, the indulgent Parent the affectionate Brother and the sincere Friend were the prominent features in a character uniformly amiable. His charities were not those of ostentation but of feeling. His strict discharge of his professional duties that of principle, not form. As he lived in the possession of the esteem of his parishioners, so he has carried to the grave their regret.

* *Cambridge Portfolio*

† Michell and Cavendish were great friends, and made geological excursions together. Their acquaintance was, perhaps, first made at Cambridge. Cavendish entered at Peterhouse on November 24th, 1749, but left without taking a degree.

‡ See *Men of Letters and Science of the time of George III.*

§ *Quar. Rev.*, Vol. LXXVII., *passim.*

"He died the 21st April, 1793, in the 69th year of his age.

"And in the same spot united in death as in life, the body of Gilbt. Michell. Esqr., Brother to the above, lies interred, a gentleman whose peculiar urbanity of manners and suavity of temper, attracted the love and esteem of all who knew him. In the conscientious discharge of all those offices which characterize the honest and good man. He was the counterpart of the above: united in the closest bonds of fraternal love. The union of virtuous and philanthropic sentiment was not less strict. For more than twenty years He was an ornament to and a valuable resiant [!] of this parish. He died the 15th Novr., 1792, aged 66.

"Gratitude and affection have erected this tablet to their memory." *

BYE-PRODUCTS *versus* WASTE-PRODUCTS.

By VAUGHAN CORNISH, M.Sc., F.C.S.

THE well-known Leblanc process for making soda came into use about one hundred years ago. Till its introduction the alkali required in various manufactures was nearly all obtained from the ashes of plants. Potash, the alkali obtained from land plants, was more plentiful than soda, which is obtained from the ash of sea plants. The sodium of the soda yielded by sea plants is contained in the sea in the form of common salt. By Leblanc's process the soda is prepared directly from common salt, thus avoiding the tedious and uncertain collection of sea-weed. The first impetus which the new method received was in 1793, when France found herself deprived of alkali, which used to come chiefly from Russia and America. How serious was the inconvenience thus caused may be readily imagined, since the manufacture of soap and of glass is dependent on a supply of alkali.

Leblanc's process for preparing soda direct from salt relieved France from an industrial difficulty. It also changed the relative importance of the two alkalies, making soda much cheaper and more plentiful than potash. Leblanc himself died in a French workhouse, but manufacturers on this side of the Channel were more fortunate, and many of them accumulated wealth as rapidly as the iron-masters did in later times. The demand for soda was unlimited, and the sole object of the manufacturer was to produce as much of the article as possible. The materials for the manufacture are the following, viz.:—first, salt and sulphuric acid, which react on one another, producing "salt cake" (sulphate of soda) and hydrochloric acid gas; secondly, limestone and coal, which, being heated with the sulphate of soda, produce the required alkali, carbonate of soda, more commonly known simply as "soda." In this second reaction the final products, besides soda, are sulphide of calcium and carbonic acid gas. Leaving out of account this last substance, the product of all combustion, we see that in the manufacture of soda the waste products are, or were, two, viz.:—hydrochloric acid gas and sulphide of calcium, or "alkali-makers' waste." The former substance was allowed to escape into the atmosphere, killing all vegetation for miles around, whilst the latter accumulated in vast heaps in the neighbourhood of the works, constantly undergoing decomposition through the action of air and moisture, and poisoning the atmosphere with fumes of sulphuretted hydrogen, besides polluting the streams with the poisonous drainage of the decomposing mass. The alkali-maker cared for none of these things, and the Government found it necessary to legislate in order to protect the health and property of the manufacturer's neighbours. The manufacturers were compelled to condense and retain the fumes of hydrochloric acid, though the decomposition from the "waste" continued to be a nuisance to the neighbourhood. The alkali-maker being obliged to go to the expense of collecting the hydrochloric acid, set to work to compensate himself for the outlay involved, by utilizing the formerly waste product hydrochloric acid, which now became a bye-product of the manufacture. More recently a process has been introduced for recovering the valuable sulphur from the sulphide of calcium. Employing the same materials as formerly (salt, sulphuric acid, limestone, and coal), the principal products of the modern manufacture are three instead of one as formerly—that is to say soda, hydrochloric acid, and sulphur, instead of soda only. It is a singular fact that at the present time the Leblanc manufacturer produces the soda at a loss, while the bye-products yield a profitable return, sufficient to pay him the interest on the capital which is locked up in the huge plant of the alkali works. From the hydrochloric acid chlorine is now made, and the chlorine gas, passed over dry slacked lime, forms the bleaching powder which is used in enormous quantities for whitening cotton and other goods. Large quantities are also used for disinfecting purposes; the recent cholera scare, by the way, sent the price up in a manner most satisfactory to the makers. At first one of the chief items of expense in the manufacture of bleaching powder was the consumption of manganese dioxide, employed to set free chlorine gas from the hydrochloric acid. This is now "regenerated" by Weldon's process, in which the chloride of manganese formed in the reaction is acted upon by air and steam in the presence of lime, the final "waste" material being calcium chloride. Similarly, in Chance's process for the recovery of sulphur from the alkali-makers' waste, the same substance, calcium chloride, is the final waste product. Chance's process is conducted in two stages: first, the partial oxidation of the sulphide of calcium by air and steam, and secondly, the decomposition of the oxidized product by hydrochloric acid, in which sulphur separates out and calcium chloride is formed. Calcium chloride is a material for which there are but few applications, and it is practically "waste." The loss of calcium is not of importance, since abundance of the useful compounds of this element can always be obtained from natural sources, such as limestone. But the chlorine is valuable, and many efforts have been made to recover this element from calcium chloride. It is quite possible to set the gas free, but hitherto a sufficiently cheap and simple process has not been found, although much attention has been devoted to a problem the solution of which may yield a large fortune. Calcium chloride is indeed the final waste product in a large number of chemical processes.

We have mentioned that the soda made by the Leblanc manufacturers is now produced at a loss. This is due to the introduction of another method of manufacture, known as the ammonia-soda process, in which a purer product, commanding a higher price, is simply and quickly obtained. In this process carbonic acid gas is passed through water which contains, dissolved, both common salt and ammonia. Soda, in the form of the bicarbonate, is deposited from the solution in crystals, the carbonic acid uniting to the base

* The Rev. F. R. Grenside, M.A., Queen's College, Cambridge, the present Rector of Thornhill, has kindly furnished me with this copy of the memorial tablet. In sending it he remarks that it is impossible not to be struck with the fine style in which it is written. It is likely that the mason is responsible both for the punctuation and the spelling.

soda, and the ammonia taking the chlorine, forming ammonium chloride, which remains in solution. The conditions under which the soda is produced in this method of manufacture are very favourable to the formation of a pure product. Both by the solution of the common salt and by the subsequent crystallization of the carbonate of soda, impurities are eliminated. In the Leblanc process, on the other hand, impurities accumulate, and their subsequent removal involve time and expense. The second stage of the Leblanc process, as we have already stated, consists in the decomposition of sulphate of soda by means of limestone and coal. All these solid substances are heated together on the floor of a furnace. After cooling the mass is treated with water, which dissolves out the carbonate of soda and leaves behind the calcium sulphide and any unburnt coal. But the solution contains a good many other substances besides carbonate of soda, such as sodium sulphide and sodium thiocyanate, besides large quantities of caustic soda.

In spite of all disadvantages under which he suffers in competition with the ammonia-soda process, the once envied "Leblanc" manufacturer is still just able to maintain his business by the sale of the bye-products of the manufacture, made from what was formerly waste material. A full account of the various inventions by which the utilization of the bye-products has been brought about would fill a volume, and does in fact actually fill many volumes of technical literature. Chemistry as an industrial art is best studied in the districts where alkali is manufactured and is used for glass and soap making, in South Lancashire for instance, and in the districts such as the neighbourhood of Middlesbrough, where iron and steel are made. In both districts one may study the successive improvements by which the most is made of products formerly considered "waste." In the blast furnace, as was pointed out in the last number of Knowledge, the constant endeavour has been to utilize the energy of the half-burnt carbon so as to diminish the consumption of the raw material coal. In the Leblanc process the economy has been effected by the preparation of useful substances from useless materials. The iron-makers, like the alkali-makers, had hard times to meet, and they tided over the hardest time by reason of the improvements which care and foresight led them to introduce in the manufacture. The alkali-maker only turned his attention to economy of material when legislation compelled him to do so; but in his case, and in that of the more provident producer of iron (and, we might add, of manufacturers in all branches of industrial chemistry), prolonged prosperity has only been attained by minimizing wastefulness as much as the wasteful tendency of all natural change permits.

THE MOVEMENTS IN THE LINE OF SIGHT OF STARS AND NEBULÆ.

By Miss A. M. Clerke, *Authoress of "The System of the Stars" and "The History of Astronomy during the Nineteenth Century," &c., &c.*

The movements of the heavenly bodies directly towards or from the earth might well have seemed an element placed by the nature of things outside the scope of terrestrial enquiries. Yet it has proved capable of determination; not merely in a rough and general way, but in many cases with a precision answering to the strict demands of modern calculating astronomy. Hope has indeed been left a long way behind in this branch of research, and *Nil desperandum* might now, with full justification, be taken as their motto by adventurous astro-physicists. Through their achievements in measuring spectroscopic line-displacements in stars and nebulæ, sidereal science is becoming rapidly revolutionized: stellar systems, which might have been judged beforehand of improbable, not to say impossible construction, are found in full working order, and distributed with no extreme scarcity through space: promise is afforded of completely disentangling the translatory motion of the sun from the confused flittings of the stars; and materials are being provided for investigating their dynamical relations with greatly improved prospects of success.*

The theoretical possibility of spectroscopically measuring motion was recognized many years before it could be realized. First of all, Christian Doppler, in 1842, announced the fundamental principle that light-waves are altered in refrangibility, as sound-waves in pitch, by the recession or approach of the emitting bodies. But his use of the principle was utterly futile. The curious notion somehow laid hold of him that it would account for the varied colours of the stars; as if the invisible rays at either end of every ordinary stellar spectrum were not at hand to restore the chromatic balance, which might otherwise be infinitesimally disturbed by motion. Alone among his contemporaries, Fizeau perceived, in 1848, the real capabilities of the method; he saw that the shift of a spectrum might serve to determine radial velocity, and referred to the Fraunhofer lines as the natural indices to the amount of the shift.

Nobody, however, at that time knew anything about the origin of the Fraunhofer lines, nor was it by any means certain that they possessed the essential fixity needed to make their incidental displacements of critical value. Fizeau's suggestion had sunk into oblivion when, after twenty years, Dr. Huggins practically demonstrated its importance. His success was decisive. The effectiveness in sidereal investigations of the spectroscopic method of determining radial motion was thenceforward generally admitted, and its application became one of the recognized tasks of astronomers.

But the requisite visual observations are hampered by very great difficulties. Starlight does not readily endure high dispersion. Unless when concentrated by telescopes of immense apertures it ceases, as it were, to be *articulate* if enfeebled through extension. Its characteristic rays—the very objects of measurement—fade into evanescence. Without high dispersion, on the other hand, their minute displacements are apt to get disguised or exaggerated by atmospheric tremors. Both these inconveniences can fortunately be obviated by substituting photographic for direct

[* Miss Clerke takes a much more hopeful view of the accuracy of recent determinations of motion in the line of sight than I feel that I am able to take. Up to the present time there have been very serious differences in the estimates made by the best observers as to the amount and even as to the direction of motion, towards or away from us, of stars and nebulæ. Dr. Vogel has no doubt succeeded in eliminating some sources of error due to flexure of the spectroscope and other causes, and it will be welcome news to all interested if his estimates of the amount of his errors are confirmed by the observations of other spectroscopic observers. But at present the results independently obtained in this line of research differ more from one another than the determinations of parallax differ. It therefore seems to me unsafe to build on such foundations when forming conclusions as to the stellar universe. My remarks do not apply to measures of the relative motions of adjacent stars in the line of sight, such as led to the discovery by Prof. Pickering of binary stars too close to be visibly separated in the largest telescopes. In spite of what I have said, I feel that Miss Clerke's statement of the case for Dr. Vogel is very interesting.—A. C. Ranyard.]

[What Mr. Ranyard says in his note is undoubtedly true as regards a good many of the Potsdam stars; but the only two which have been independently measured with adequate means have given identical results with those obtained by Dr. Vogel. These are, as mentioned in the article, Arcturus and Aldebaran.—A. M. Clerke.]

observation. The extreme sensitiveness of modern plates causes a little light to go a long way in producing legible impressions; and these impressions firmly hold their ground against aerial vibrations as regards position, suffering only some slight prejudice in point of definition.

Since 1887, accordingly, Dr. Vogel, aided very effectually by Dr. Scheiner, has been engaged in perfecting the "spectrographic" method, and with so prosperous an issue as to ensure its wide adoption. The accuracy attained, in fact, is, under favourable circumstances, of so high an order as to give hope of an eventual reactive effect in correcting the values at present assigned to the dimensions of the solar system; for the orbital travelling of the earth, now in a positive, now in a negative direction, necessarily records itself, in varying but determinable proportions, on each plate exposed to the prismatic rays of the stars. The measured displacements have then to be cleared of its effects, their residue giving the constant quantity of stellar movement towards or from the sun. The terrestrial speed is of necessity, at the actual stage of the enquiry, taken as known, the star's movement being treated as unknown. But with some further advance in exactitude, this relation may in the future be inverted, and the earth's rate of circulation determined anew by subtracting from the gross amounts of measured velocity the known movement of some star. Thus the great astronomical unit of the sun's distance will perhaps before long come to be more satisfactorily ascertained, through seemingly insignificant modifications of stellar spectra, than it has yet been by means of transits of Venus, oppositions of Mars, or laborious experiments on the swift travelling of light.

Spectroscopic determinations of motion might almost be said to be unconditioned by either time or space. They are independent of distance, provided only the concentrated rays they deal with retain sufficient intensity for the purposes of analysis; and they give results absolutely, and at once. "Proper," or thwartwise motions have to be gathered in slowly, decade by decade, even century by century; but end-on motions put no strain of waiting on human impatience. Five years have hence sufficed for the execution of the immediate part of the task undertaken at Potsdam. Those five years, moreover, were mainly spent in the development of untried methods, in the invention and construction of novel apparatus. The work was of the pioneering sort, and could not proceed with the same rapidity as if the path of its progress had been long ago smoothed and straightened. Its results, now published in detail,* claim a peculiar and unique interest. They are the first of their kind, and they imply the promise of much more than they actually give. Their substantive importance, nevertheless, is great. The fifty-one stars measured are all those suitably situated, and bright enough for the grasp of a 12-inch refractor. With a larger instrument, in course of construction, Dr. Vogel hopes to deal similarly with a longer list of fainter stars, and thus to collect sufficient materials for solving the problem of the sun's motion in space—above all, one may add, for fixing the rate of that motion, so far entirely unknown.

The first notable point about the motions in line of sight just now published is their moderate speed. The average velocity of the collection comes out no more than 10·4 English miles per second. And this, it must be remembered, includes the effect of the solar translation, which may accordingly be surmised to progress at a slower pace than had of late been more or less conjecturally assigned to it. The highest velocity determined at Potsdam belongs to Aldebaran, which recedes from us by thirty miles a second, γ Leonis coming next with twenty-four miles of approach. There is a total absence of startling records; nicety of observation has been strongly operative in slackening pace. Dr. Vogel's stars travel at moderate, decorous, and explicable rates. No such celestial projectiles occur among them as μ Cassiopeiæ or Groombridge 1830, no correlatives even of ζ Toucanæ or 40 Eridani. Moreover, the average rate of advance along great circles of the sphere of fifty-one stars with ascertained proper motions, and at more or less reliably known distances, appears to be about thirty-four miles a second, or more than thrice the average radial speed of an equal number of—it may be well to remark—much brighter stars. The disparity is striking, but it may possibly be reconciled by further research.

The "goal of the sun's way" may now be placed, with some confidence, in the neighbourhood of the brilliant Vega—let us say, taking the mean of M. Oscar Stumpe's recent determinations, in R. A. 285°, Dec. +38°. In that quarter of the heavens, then, movements of approach must considerably outweigh movements of recession, while, in the opposite quarter near the sun's "anti-apex," the relation is doubtless inverted. But here the stars are, for the most part, invisible in northern latitudes, so that negative motion —motion, that is to say, serving to curtail distance— ought, on the whole, to predominate in the Potsdam list. Of the stars included in it, accordingly, thirty-one are approaching, only twenty receding from our system; while the proportion of stars possessing a negative to those showing a positive velocity above the average of 10·4 miles a second in either sense, is eleven to seven. The inequality would, however, presumably be removed if the observed stars were fairly divided between the northern and the southern hemispheres.

The adopted velocities in the Potsdam table are the means of independent measures by Drs. Vogel and Scheiner. These, in most cases, agree well; in some, they are nearly identical. Yet discordances are not wholly absent. Little reliance, for instance, can be placed on the ostensible result for γ Cassiopeiæ, which, according to Vogel, recedes from the earth at the rate of 2·5 miles, yet was found by Scheiner to have an approaching speed of 6·9 miles a second. Both observers, on the other hand, agreed as to the negative direction of the movement of Sirius; but the velocity assigned by Vogel was of 8·4, by Scheiner of 12·5 miles a second. This discrepancy is indeed minute compared with those which affected the earlier visual estimates of this star's motion. They wore, too, a delusive aspect of periodicity difficult to be accounted for by the mere vagaries of instrumental error. First, the recession attributed to the star gradually diminished its rate between 1868 and 1882, from twenty-five to two miles per second; then the direction of motion seemed to become reversed, and approaching velocities, increasing with the same semblance of regularity, were registered. The change, although ten times greater than could be accounted for by the known revolutions of Sirius round its dim companion, appeared to proceed with such steadiness and consistency that few questioned its genuineness. Yet, it is now fully admitted to have been altogether illusory. The Sirian system, there can be little doubt, is transported towards the solar system at about the same rate of ten miles a second with which it moves across our visual line. But the sun is travelling away from the star, which is situated in the immediate vicinity of the solar anti-apex. Its radial motion hence represents a gain upon the sun. Besides the measurable ten miles a second, Sirius possesses,

* "Publicationen des Astrophysikalischen Observatoriums zu Potsdam." Von H. C. Vogel. Band VII., Theil I. Potsdam, 1892.

then, nearly the whole of the solar speed, and the sum has to be further increased by being compounded with a visible or tangential movement of ten miles a second ; so that the Dog-star traverses space probably about twice as quickly as the sun.

Arcturus furnishes another notable instance of the surrender, on closer enquiry, of a fictitious velocity. Its original seeming approach of fifty-five miles has dwindled, at Potsdam, to 4·6 miles a second—a result in all but perfect agreement with that independently obtained by Professor Keeler with the great Lick refractor. His measures were made upon the D-lines in the spectrum of the star ; and a similar determination of Aldebaran by Mr. W. W. Campbell shows recession to the amount of 30·5 miles, the Potsdam photographs giving 30·2 miles per second. The former inferiority of the visual to the chemical method of ascertaining the radial movements of stars thus seems at last abolished ; though not always or everywhere. The Lick atmosphere and the Lick telescope form a hitherto unmatched combination, and the signal advantages conferred by them have been illustrated by an important discovery. Owing to the enormous supply of light at his command, Professor Keeler was able to execute micrometrical measures in nebular spectra of the fourth order, the dispersion, equal to that given by twenty-four prisms of 60°, being effected by a Rowland's grating of 14,438 lines to the inch. The upshot was the detection of motion-displacements of considerable magnitudes. Until then nebulæ had appeared absolutely stationary ; they yielded no certain sign, telescopic or spectroscopic, of mobility in any direction. The removal of this seeming anomaly constituted an advance of no small moment. The outcome of Professor Keeler's experiments was more decisive than could have been anticipated. The mean velocity towards or from the earth of the eleven nebulæ measured (including that in Orion), proved to be once and a half times that of the Potsdam stars, or sixteen miles a second.* Eight of these showed movements of approach, only three movements of recession. The disparity, however, was doubtless due to their situation mainly in the hemisphere *in front* of the sun. The most rapid traveller amongst them is the celebrated planetary in Draco (N.G.C. 6543 = H. IV., 37), found by Professors Holden and Schaeberle to possess a curious helical structure. Prolonged observations have as yet elicited from it no trace of proper motion ; yet its rate of transport in our direction turns out to exceed forty miles a second. The swiftest motion of recession—twenty-eight miles a second—belongs to a small stellar nebula (N.G.C. 6790) : the well-known "Saturn planetary" in Aquarius advancing at a nearly equal rate. So far as these preliminary trials go, then, nebular take the lead of stellar movements in the line of sight.

The Orion nebula recedes from the sun, or we should perhaps rather say the sun leaves it behind at the rate of 10·6 miles a second. A solar velocity of 13·6 miles, directed towards a point some ten degrees east of α Lyræ, would completely account for the observed line-displacements in the spectrum of that amazing object, and allow us to adopt the plausible hypothesis that the vast system of which it forms the nucleus exists in a state of comparative rest. It may be worth notice that the three stars, α, β, and ζ Orionis, share the virtual retreat from the earth of the chaotic mass adjacent to them on the sphere, and perhaps not disconnected from them in space. Another example of related motion in the stellar members of a nebulous system is most likely met with in α and γ Cygni. Both stars are pretty clearly shown in M. Wolf's photographs—as the readers of Knowledge for October, 1891, may remember—to be involved in the outskirts of the same extensive nebula ; and they are found to be endowed with what may be fairly called a combined movement towards the sun of four or five miles a second. If this were purely a transferred effect of the solar translation, it would imply for it a speed of only 5·3 miles a second, which seems unreasonably small. The alternative supposition is perhaps to be preferred—namely, that the sun tends to overtake the star-and-nebula system, which travels in the same direction, but at a slower pace. Such points, however, can scarcely yet be profitably debated ; they can certainly not be decided until a complete solution of the problem of the solar movement is at hand, based on materials derived in equal measure from the southern and northern hemispheres. The nature of the required data could not be better exemplified than by the specimen-lists of radial velocities provided from Potsdam and Lick.

* Reducing to the true wave-length of the chief nebular line, as subsequently fixed by Keeler himself at 5005·05 of Angstrom's scale.

Notices of Books.

La Planète Mars et ses conditions d'habitabilité. Par Camille Flammarion. (Gauthier - Villars, Paris.)—M. Flammarion has presented the world with a very important monograph on the planet Mars, to which he has devoted much time and labour. The volume, which is a large octavo and extends to 600 pages, contains over 300 beautiful woodcuts, in which M. Flammarion has reproduced 580 drawings of the planet by the principal observers of Mars. Nothing of any importance seems to have escaped him. Beginning with Fontana's very rough sketches made in 1636, and ending with drawings made during the present opposition, in addition to sketches and drawings made by other observers, he has also given many beautiful drawings of his own, and admirable maps of the chief planetary features. M. Flammarion is of opinion that Mars is or may be inhabited, and that a very large number of the canals and other details delineated have a real existence, and are not due to any optical illusion. But some of the drawings, especially those of the Schiaparelli type, which he gives are very diagrammatic, and so unlike the projection of markings on a sphere that they forcibly suggest that the canals and straight dark lines must be the result of some optical illusion.

Jupiter and his System. By Ellen M. Clerke. (Edward Stanford, Cockspur Street, London.)—Miss E. M. Clerke has taken the opportunity of the present very favourable opposition of Jupiter to give the public a lucidly-written account of what is known about the physical condition of the giant planet. Miss Clerke, like her better known sister the authoress of "The System of the Stars," writes very plainly and interestingly, and her little book is likely to turn the eyes and the thoughts of a great many fresh observers to the changes continually going on upon this magnificent planet. Many of these changes and other phenomena observable on the planet may be easily seen with small instrumental means, especially when Jupiter is in opposition. Like Mars, Jupiter is most favourably situated for observation when it is in the opposite quarter of the heavens from the sun. It crosses the meridian at midnight, and the illuminated disc is turned full upon us. The earth is then between the sun and Jupiter, and when Jupiter is near to perihelion, as at the present opposition, the earth's distance from the planet is only 369 million miles, whereas when Jupiter

is in aphelion the earth's distance from the planet is 411 million miles. Consequently, at an October opposition, when Jupiter is in perihelion, he appears nearly 40 per cent. brighter than at an April opposition when he is in aphelion. Jupiter presents a great many analogies with the sun. Its density compared with water is 1·378, whereas the density of the sun is slightly greater, viz., 1·444. As in the case of the sun's photosphere, the equatorial regions of Jupiter revolve a little more rapidly than the regions more to the north and south, thus proving Jupiter to be, like the sun, mainly a gaseous body. Its surface, however, presents a very different appearance from the sun's surface. The disc of the planet is crossed by dark belts of cloud which are continually changing their form. On the belts and between them are seen black, white, and reddish spots. Miss Clerke gives an interesting account of one of the largest and most permanent of these, known as the "Great Red Spot." It was first observed by Prof. Pritchett, of Glasgow, Missouri, in 1868, and soon attracted considerable attention. After three years of conspicuous brightness its colour began to fade, till in 1882-83 it had almost ceased to be visible. In 1885 it had begun to recover in brightness and then showed as a faint pink oval ring, with its centre occupied by a white cloud, which in the following year so extended as almost to obliterate its outline. The veil has, however, since cleared away, and left it as an elongated brick-red spot which moves somewhat slower than other markings in the same latitude. The different velocities gave occasion for an interesting observation by Mr. Stanley Williams on the conjunction of the red spot with a dark one, which is a fairly persistent feature of the planet. Its size was such that it would cover about half the shorter diameter of the greater spot if it should pass above it, and its relative speed was sufficient to carry it across its length in two months. The question how it would behave under these circumstances was one of considerable interest, for, its transit above or below the red spot would show which of these classes of objects occupied the higher level in the planet's atmosphere. Miss Clerke remarks that the black spot contrived to baffle expectant astronomers by doing neither. It took a third course and went round the obstacle, swerving away to the south and coasting the southern rim of the red spot. We can heartily recommend Miss Clerke's little shilling monograph, and hope that it will start many a reader in the pleasant paths of observational astronomy.

Mr. Barnard has kindly sent for reproduction in Knowledge some beautiful photographs of Swift's comet, showing remarkable structure in the coma and tail, and proving that the structure changed very rapidly from day to day.

One of the results of Mr. Burnham's departure from the Lick Observatory is that a Chicago millionaire, Mr. Charles T. Yerkes, has undertaken to present the University of Chicago with a refractor of 45 inches in diameter—that is, the object-glass will have a diameter greater by 9 inches than that of the great Lick telescope. He has commissioned Mr. Burnham and Prof. Geo. E. Hale to order the instrument, the object-glass of which will be made by Messrs. Alvan Clark. It has been truly said that with these large instruments the man at the small end of the telescope is the most important part of the equipment. With such a telescope, and such an observer as Mr. Burnham to use it, Chicago ought to make its mark in the history of astronomy.

Letter.

[The Editor does not hold himself responsible for the opinions or statements of correspondents.]

To the Editor of Knowledge.

Sir,—In the interesting article in the last number of Knowledge on "What is a Nebula?" Mr. Ranyard draws a vivid conception of the extreme tenuity of nebular matter. It seems to involve, however, a question which, to my uninformed mind, is very mysterious. I assume the following statements to be substantially correct:—

1st. The spectrum of such nebular matter shows it to be, in a great number of cases, gaseous.

2nd. Its luminosity means also high temperature.

3rd. The temperature of ethereal space is immensely lower than anything we have experience of.

Under such conditions, how is it possible that this nebular matter can maintain its luminosity and its temperature for a period indefinitely long? If it is true that all substances cool down by radiation into colder space, one would think that a nebula ought to disappear almost as soon as it is formed, instead of streaming out its light for ages.

Middlesbro', Oct. 22nd, 1892. R. H.

[We are hardly in a position to say that luminosity necessarily indicates a high temperature. The light of the glow-worm is evidently produced at a low temperature—and the auroral light is given out by matter in the cold regions of our upper atmosphere. It may be that the nebular matter is not all glowing, but that only the matter in isolated and widely separated regions is caused to glow by electrical discharges, or the impact of meteors. Personally, I do not incline to the latter theory, or to the theory of illumination by electrical discharges, but I will explain my ideas in a further paper on nebulæ, for which there is not room in the present number.

I have to thank Mr. Walter Sang for pointing out a numerical mistake in my last article on nebulæ. The density of atmospheric air at the sea-level at standard temperature and pressure is $\frac{1}{769 \cdot 8}$ of the density of water, not $\frac{1}{846}$. And the velocity of our sun in a circular orbit about the Orion nebula at the distance of α Centauri, assuming the conditions as to density and volume of the nebula mentioned in the last number, would be 7·5 miles, not 18 miles a second. If the nebula had a millionth of the density of atmospheric air, the velocity of our sun in a circular orbit about the nebula, if it were situated at the distance of α Centauri from us, would be 75·08 miles a second, and whatever the distance of the nebula under the conditions assumed, the angular velocity of our sun in a circular orbit, as seen from the nebula, would be constant. Similarly, the angular velocity in any elliptic or parabolic orbit would be constant, and the apparent velocities of stars moving under the control of gravity in the neighbourhood of the nebula, as seen from the earth, would be independent of the distance of the nebula.—A. C. Ranyard.]

THE OLDEST MAMMALS.

By R. Lydekker, B.A.Cantab.

Up to the year 1818 it was a generally received axiom of geology that mammals were totally unknown before the tertiary period; and that period was consequently designated the age of mammals—a name, by the way, which is still perfectly appropriate, if taken to imply that these animals then, and then only, became the dominant inhabitants of the world. In that year, however, the illustrious Cuvier,

during a visit to the museum at Oxford, was shown two minute jaws, carrying a number of cusped teeth, which had been obtained in the neighbouring quarries of Stonesfield, from the rock known as the Stonesfield slate, belonging to the lower part of the great jurassic, or oolitic, system. After careful examination, the French anatomist pronounced confidently that these two tiny little jaws, neither of which exceeded an inch in length, were those of mammals, and he further suggested that they would prove to belong to a species of opossum. Although this opinion was given in the year 1818, it does not appear that it was published till the year 1825, when the second edition of the fifth volume of the immortal "Ossements Fosiles" saw the light. In publishing this epoch-making notice of the occurrence of mammals in the secondary period, Cuvier, with the usual caution of naturalists, was careful to add the proviso that everything depended on whether the specimens he saw had really been obtained from the Stonesfield slate. Unfortunately, there does not appear to be any record stating by whom, or at what date, these original specimens—now forming some of the most valued treasures of the Oxford Museum—were obtained from the Stonesfield slate; but that they did come from that formation is perfectly certain. Indeed, other specimens have been subsequently obtained from the same beds, showing certain characteristic Stonesfield shells embedded in the fragments of rock in which the mammalian jaws are contained. Here we may mention that the Stonesfield slate is the equivalent of the lower portion of the great, or Bath, oolite—a deposit which is separated from the underlying lias by the beds known as the inferior oolite. Consequently, the mammal-yielding beds are separated from the rocks of the tertiary period, not only by the immense series of cretaceous deposits (chalk, gault, greensands, and wealden), but likewise by a large thickness of those belonging to the jurassic system, such as the Purbeck and Portland oolites, the Kimeridge clay, the coral rag, and the Oxford clay.

Fig. 1.—One half of the lower jaw of a Stonesfield Mammal; twice natural size. The restoration of the front teeth is conjectural.

Needless to say, no sooner was the existence of mammals in the Stonesfield slate announced than it was received with a howl of incredulity. First of all it was attempted to show that the specimens themselves did not come from Stonesfield; and no sooner was this objection knocked on the head than doubts were raised as to the jurassic age of the Stonesfield slate itself. These, however, were equally soon disposed of, and the only thing then remaining was to dispute the mammalian nature of the fossils. This task was undertaken by the French naturalist, De Blainville, who attempted to show that the mammalian character of the specimens was not proved by the double roots of their molar teeth. In the course of the argument, great stress was laid on the circumstance that two-rooted molars were found in a gigantic animal from the eocene of the United States, then known as *Basilosaurus*, and regarded as a reptile. De Blainville was, however, here treading on very dangerous ground, for it subsequently turned out that *Basilosaurus* itself was really a mammal, which is now generally placed among the whales, under the name of *Zeuglodon*. The correctness of Cuvier's original determination was thus in the end triumphantly sustained, and the existence of jurassic mammals became henceforth an established fact in geology, although the suggestion that these fossils belonged to opossums was, of course, unfounded.

Passing on to the consideration of the specimens themselves, we find that the great peculiarity of the jaws of these Stonesfield mammals (for one of which De Blainville proposed the name of amphithere) is the excessive number of their cheek-teeth, a feature now paralleled (as we have mentioned in an earlier article on "Pouched Mammals") only in the little banded ant-eater of Australia. This multiplicity of teeth is well shown in the jaw represented in Fig. 1, which is preserved in the museum at York, and shows upwards of nine cheek-teeth still remaining, whereas in practically all existing mammals with complex teeth, except the banded ant-eater, the number does not exceed seven. Other jaws were, however, subsequently discovered in Stonesfield, in which the number of cheek-teeth was considerably less; but one of these later specimens (described as the phascolothere), revealed the important fact that there were four pairs of front or incisor teeth in the lower jaw. Now since (as stated in the article cited) it is only among pouched mammals, or marsupials, that more than three pairs of incisor teeth are found, while the banded ant-eater, with its numerous cheek-teeth, is a member of the same group, it became a very natural conclusion that the Stonesfield mammals were likewise marsupials. Support was lent to this conclusion by the circumstance that, with the exception of the egg-laying mammals, or monotremes (to which a special article in Knowledge has likewise been devoted), the marsupials are the lowest of all living mammals. And indirectly some further support to this view is afforded by the fact that Australia still retains other forms of animal life allied to those which were living in Europe during the period of the Stonesfield slate. For instance, it is in the Australian seas alone that there still survives the solitary representative of the beautiful genus of bivalve shells known as *Trigonia*, which were so especially abundant in the oolites; while it is also there alone that swims the Port Jackson shark, whose mouth is armed with a pavement of crushing teeth, recalling those of many of its jurassic forerunners. Moreover, in the presence of numerous cicads among its flora, Australia again recalls the jurassic epoch of Europe; and it has accordingly been suggested that modern Australia might be regarded as a kind of direct survival from jurassic times. Before, however, we can say anything more as to the affinities of the Stonesfield mammals, we must turn our attention to subsequent discoveries of mammalian remains in other formations.

The first of these discoveries was made in the year 1847, by Professor Plieninger, of Stuttgart, who obtained certain minute teeth from a bone-bed near that town belonging to the upper part of the triassic period, which were declared to be mammalian, and for the owner of which the name *Microlestes* was proposed. Now, as the trias lies below the lias, the existence of mammalian life was by this discovery

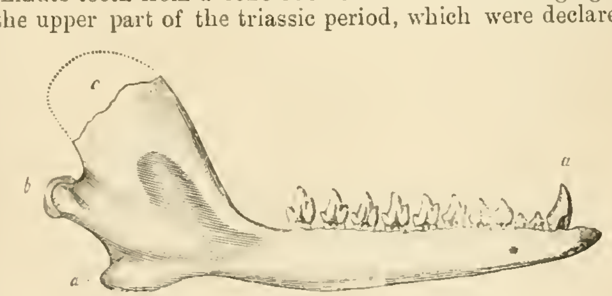


Fig. 2 Lower jaw of an American Jurassic Mammal; twice natural size. (After Marsh.)

carried back at one bound very nearly to the commencement of the secondary period. Subsequently, mammalian remains were obtained from the trias of Somerset, which proved to belong to the same genus as those from Stuttgart, while others of a different type were found in the equivalent deposits of North America.

A little later, the year 1854 was made memorable by the first discovery of mammalian remains in the freshwater Purbeck strata of Dorsetshire, belonging to the very top of the jurassic system: from which formation in subsequent years a vast number of such remains were obtained, through the energy of the Rev. P. B. Brodie and the late Mr. Beccles. All these specimens were obtained from a single bed, and many of them indicated forms more or less closely allied to those from Stonesfield and Stuttgart. It was thus shown, once for all, that mammalian life must have been locally abundant throughout the jurassic period. This conclusion was subsequently amplified by the discovery in the upper jurassic rocks of North America

Fig. 3.—Lower jaw of *Triconodon*; half natural size. (After Marsh.)

of a whole host of small mammals very closely allied to those from Dorsetshire, a large number of which have been described by Prof. O. C. Marsh, some of whose figures are here reproduced. Many of these small jurassic mammals (Figs. 2 and 3) were evidently carnivorous, and such carnivorous forms exhibited two distinct types of dentition. In one of them (Fig. 2) there was a numerous series of cheek-teeth behind the tusk, or canine (*a*), each of which carried three cusps arranged in a triangle; while in the other type (Fig. 3) the cheek-teeth were fewer in number, and had the three cusps on their crowns ranged in the same line. From this peculiarity the animal to which the second type of jaw belonged was appropriately named *Triconodon*. The first type corresponds to the amphithere of the Stonesfield slate, while the second is more like the phascolothere of the same formation.

In our third figure it will be observed that there is a peculiar groove (*g*) running along the inside of the jaw, and since a similar groove is found among existing mammals only in the banded ant-eater and certain other carnivorous marsupials, we have pretty conclusive evidence that *Triconodon* and its allies were really marsupials. There can also be but little doubt that the species of the amphitherian type (I avoid mentioning the numerous genera of these animals) are likewise members of the same order. It is, however, quite possible that some of the jurassic mammals of Dorsetshire and North America may be more nearly allied to that primitive group of mammals known as Insectivores, among which are included the mole, the shrew, and the hedgehog of Europe, as well as the more generalized tenrec of Madagascar, and many other peculiar creatures. All these insectivores are of a very low grade of organization, and the result of modern researches is to show that their connection with the marsupials is very close indeed. Hence it is highly likely that some of the jurassic mammals may have been the actual connecting links between the marsupials and the insectivores; and it is worthy of mention here that while marsupials at the present day linger on only in Australia and America, some of the most primitive types of insectivores are preserved to us in Madagascar, which is another refuge for animals of a low grade of organization.

There is yet another type of mammal found in the English and American jurassics, to which the *Microlestes* of the trias also appears to belong, which has given rise to a vast amount of discussion among palæontologists. These remarkable mammals

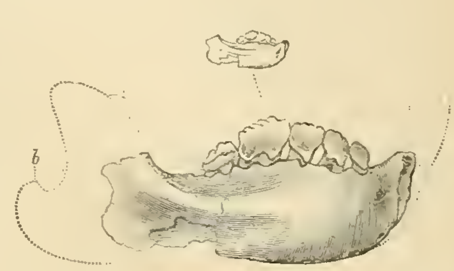


Fig. 4.—Lower jaw of *Plagiaulax;* natural size and enlarged. (After Marsh.)

are mostly of very minute size, and were long known only by their lower jaws, of which a specimen is represented in the accompanying figure; from which it will be seen at a glance that the dentition is quite different from that of either of the carnivorous types figured above. The lower teeth comprise a single large incisor (*c*), behind which were either three or four tall premolar teeth with cutting edges, and marked on the sides with a number of oblique grooves, from which the name *Plagiaulax* was taken. When unworn, these grooves extended along the whole outer surface of the teeth, but when the teeth have been long in use (as in our figure) the groovings become worn away from the sides. Behind these four premolars are two smaller molar teeth, with the summits of their crowns marked by a single longitudinal groove bounded by prominent ridges. Now it was argued at first that this very peculiar type of dentition indicated carnivorous habits in the owners thereof; but it was subsequently pointed out that the existing rat-kangaroos of Australia (of which the front of the skull is shown in Fig. 5) presented

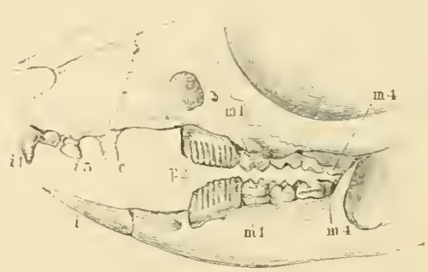


Fig. 5.—Jaws and teeth of the Rat-Kangaroo.

a somewhat similar type of tooth-structure. Thus, the last premolar tooth (*pm.*) of the rat-kangaroo has a cutting-crown marked with a number of parallel grooves; while each half of the lower jaw terminates in a single large incisor not unlike that of the jurassic *Plagiaulax*. Hence it was argued—and, in our opinion, argued rightly—that as the living form is herbivorous, the same must have been the case with the extinct one. When, however, it was also urged that the rat-kangaroo and *Plagiaulax* were closely allied animals, important differences between the two were overlooked. Thus, as will be apparent from the figures, while in the former there was but one grooved tooth, in which the grooves are *vertical*, in the latter there were usually three or four such teeth in which the grooves are *oblique*. Moreover, whereas the recent form was provided with four molar teeth (*m* 1—*m* 4), the fossil had but two such teeth; while the form of these teeth was quite unlike in the two. Hence, when we add that there are other important differences between them

(into the consideration of which it would be difficult to enter here), it will be apparent that the view of regarding *Plagiaulax* as a new ally of the rat-kangaroo was the result of attaching too much importance to resemblances, and overlooking differences. Indeed, such resemblances as do exist between the two may be regarded merely as a well-marked instance of the phenomenon known as *parallelism*, which has only lately received at the hands of zoologists the amount of attention it merits. By parallelism, we may explain, is meant a more or less marked resemblance between homologous organs or parts, which has been acquired independently, and is not the result of direct inheritance. An excellent instance of parallelism is afforded by the development of cannon-bones in the horse and the ox, such cannon-bone consisting in the one case of a single metacarpal element, and in the other of two such metacarpals fitted together. Taking it, then, as proved that *Plagiaulax* is not a near ally of the rat-kangaroo, we have to consider whether it can be affiliated to any other group of existing mammals. Before doing so, we have, however, to mention that there are certain other secondary mammals allied to *Plagiaulax*, in which the whole of the cheek-teeth are like the true molars of the latter. We have already stated that in *Plagiaulax* the lower molars had a median longitudinal groove, and it may be added that the ridges bordering such grooves are surmounted by a number of small tubercles. In the upper jaw, if we may judge by some allied genera, the molars had three such tuberculated ridges, separated by two grooves. Similar molars occur in the skull represented in Fig. 6, which is that of a mammal discovered a few years ago in the secondary rocks of South Africa, and named by Sir R. Owen *Tritylodon*; but it will be noticed that there is no trace of the cutting and obliquely-grooved premolar teeth of *Plagiaulax*, the premolars being like the molars. Detached molars of similar type have been found (as mentioned in our article on "Egg-laying Mammals") in the trias of Stuttgart, and others occur in the Stonesfield slate. Moreover, in Dorsetshire and North America there are certain nearly allied mammals (*Bolodon*) in which the upper molars have only two, in place of three, longitudinal ridges of tubercles. These forms, if other proofs were wanting, clearly show, indeed, that the resemblance between *Plagiaulax* and the rat-kangaroo is not a genetic one. When, however, the molar teeth of the type in which there are but two longitudinal rows of tubercles are compared with the transitory teeth of the Australian duckbill (see article on "Egg-laying Mammals"), a certain resemblance can be detected between the two, which seems sufficient to indicate (as mentioned in that article) that in *Plagiaulax* and *Tritylodon* we have to do in all probability with ancient types of egg-laying mammals.

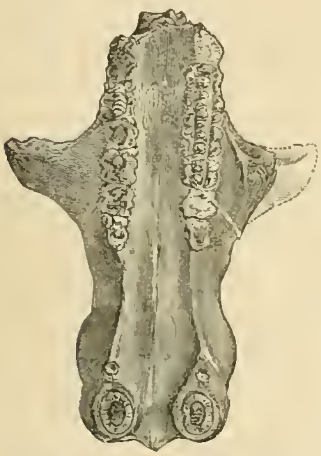


Fig. 6.—Under part of the skull of a South African Secondary Mammal; two-thirds natural size.

Till within the last few years the cretaceous period formed a complete gap as regards the history of mammals; and seeing that in Europe, with the exception of the wealden, the rocks of this system are mainly of marine origin, while some of them, like the chalk, were laid down in seas of considerable depth, this absence of mammalian remains is not to be wondered at. In the United States the condition of things is, however, very different. There the uppermost cretaceous rocks are of fresh-water origin, and constitute a series known as the Laramie, which is in intimate connection with the lower part of the tertiary, and has yielded the extraordinary horned dinosaurs, previously noticed in Knowledge in the article on "Giant Land Reptiles." From these Laramie cretaceous rocks Professor Marsh has succeeded in obtaining a quantity of teeth of mammals, although these are, unfortunately, mostly found detached. These teeth indicate mammals closely allied to *Plagiaulax* of the jurassic, and also others of a carnivorous type related to the *Amphithere*, or some of the many-molared carnivorous forms from the Dorsetshire Purbeck. Mammals of the *Triconodont* type—that is, those with the three cusps of the molars in a straight line—seem, however, by this time to have totally disappeared.

At a still later date a single tooth of the *Plagiaulax* type has been obtained from the English wealden, indicating that at that epoch the Purbeck mammals still survived in Europe, and leading to the hope that future researches will yield us further evidence of the European mammalian fauna of the wealden.

The present state of our knowledge, therefore, shows that from nearly the lowest beds of the secondary period till the close of that vast epoch, there existed a numerous fauna of small mammals distributed over a large portion of the globe, and displaying a remarkable persistence of nearly similar type. It is further evident that such of these mammals as exhibit a carnivorous type of dentition appear to be allied to the more primitive of the existing marsupials, although some of them may be more nearly related to the almost equally low insectivores. On the other hand, those which exhibit what appears to be an herbivorous modification of dental structure, if they are related to any living forms, appear to have an affinity with the modern egg-laying mammals of Australia. Now the latter, together with the marsupials and insectivores, being the lowest representatives of mammalian life at present existing, are precisely such mammals as we should naturally have expected to have been foreshadowed by more or less nearly allied forms in the secondary rocks; and, therefore, in this respect, theoretical palæontology is, so far as our present knowledge goes, precisely in accord with actual facts.

That the few triassic mammals at present known were the earliest representatives of the class cannot, however, be admitted for a moment, and we must accordingly look either to the lower triassic rocks, or to those of the underlying permian (forming the top of the palæozoic series), for the discovery of such primitive types. Should such ever be discovered, it is to be confidently expected that they will exhibit such a combination of characters common to mammals, and certain extinct reptiles and amphibians, that it will be very hard to say under what class they will have to be ranked.

Seeing that throughout the whole of the secondary period, with the possible exception of a few lowly insectivores, there is no evidence of the existence of any mammals belonging to the higher placental type (under which are included all living representatives of the class save the marsupial and egg-laying groups), the reader will naturally enquire when such higher forms first made their appearance. We answer, with the first dawn of the tertiary period; for in the very lowest eocene strata both of France and the United States there are found, side by side with small mammals allied to *Plagiaulax* and the marsupials of the jurassic and cretaceous, others, which, though still of small size, were evidently placentals. And it is very

remarkable that this first definite appearance of the higher forms of mammalian life should, so far as we know, have been contemporaneous with the disappearance of so many gigantic types of extinct reptiles, such as the dinosaurs, the fish-lizards, and the plesiosaurs, which seem to have reached the end of their term of existence at or about the close of the secondary period.

Most of these early tertiary mammals had molar teeth carrying three cusps arranged in a triangle, like their marsupial forerunners of the secondary, from which, indeed, they were probably derived; and at this comparatively early epoch the orders of mammals were but very imperfectly differentiated from one another, it being frequently difficult to decide which were carnivores and which were ungulates. A few stages later differentiation of ordinal types, accompanied by a great increase in the bodily size of their representatives, had, however, taken place; and by the close of the eocene period, as exemplified by the higher deposits of the Paris basin, most of the present orders of mammals were well defined. Thence, through the succeeding miocene and pliocene epochs, there went on a continual evolution of mammalian life, resulting in the production of giant forms like the elephant and the rhinoceros, and also characterized by the development of specially modified types like the horse and the ox, which differ so widely from their five-toed ancestors. During the same epochs antlers were developed in the deer and horns in the rhinoceroses and oxen, while pigs and hippopotami gradually acquired the enormous tusks with which their existing representatives are armed.

Seeing, then, that it was not till the advent of the tertiary period that mammals assumed the position of the dominant forms of life, Cuvier's memorable discovery in the second decade of this century that the class dated from the middle of the secondary period has in no essential respect served to dispossess the first-named epoch from its claim to the title of the AGE OF MAMMALS.

THE NEW GEOLOGY.

By the Rev. H. N. HUTCHINSON, B.A., F.G.S., *Author of "The Story of the Hills," &c.*

A GOOD deal has lately been said about "the new geology"; perhaps a few words on this subject may not be uninteresting to readers of KNOWLEDGE. It has lately been dealt with at some length by Prof. Lapworth in his address to the Geological Section of the British Association, and by Prof. James Geikie in his address to the Geographical Section. The publication of Prof. Suess' work, *Das Antlitz der Erd* (the Crust of the Earth) a few years ago, has marked an important era in geology, and a host of new and fascinating problems are suggested by that remarkable book.

Briefly, the new geology seeks to explain the curious distribution of land and water on the globe, thus connecting together geology and geography as kindred sciences. Nor does it deal only with the present state of things; for it treats of those up and down movements of the earth's crust, whereby mountain ranges are ridged up thousands of feet above the level of the sea wherein they were formed, and the world's geographical features from time to time modified. Thus, it aims at restoring the ancient geography of former periods in the world's history, and revealing the evolution of continents. But this is not all, for, with great boldness, some of the new leaders question certain theories which, till recently, were considered to be fairly established, and have suggested new ideas which may lead to important results. Perhaps our former teachers had got more or less into a groove; and there can be no harm in trying to get out of it, so long as we do not wander hopelessly from the track.

It is true that some of the problems mentioned above have, from time to time, attracted the attention of geologists; but of late years, owing to deep sea explorations, and the elaborate study of several mountain regions, so much material has been collected that they begin to feel the ground, on which once they lightly trod, somewhat more secure. Between the years 1833 and 1852 a distinguished French geologist, M. Elie de Beaumont, put forward a theory of mountain chains which attracted a great deal of attention, and was accepted by many on account of his mathematical knowledge as well as his skill as a writer. Like the illustrious naturalist, Cuvier, he believed in periods of violence or "revolutions," with long intervals of repose between them. During the latter strata were continually deposited, but during the former mountain chains were supposed to have been formed by sudden and violent upheaval. It is needless to say that Lyell successfully dissipated this notion. Elie de Beaumont endeavoured, with great skill and learning, to show that all the mountain chains thrown up during the same revolution had one uniform direction, being parallel to each other within a few degrees of the compass, even when situated in remote regions, whilst the chains thrown up at different periods took different directions. But although this bold theory broke down, it set people thinking, and therefore served to pave the way for those who came after. Now, mountains are, as it were, the backbones of continents, and have evidently a close relation to them, determining their general directions and other features; hence, problems connected with geography, either of the present or of the past, must deal with these important features. It seems to be generally accepted that mountain ranges are wrinkles, or complicated compressed folds in the earth's crust, while the broad expanses of continents are low arches, also made up of strata, and the ocean basins are supposed to be broad troughs or inverted arches. But this seems to imply that, even in the deepest depressions of the ocean, strata exist similar to those of which continental arches are composed, a conclusion which the present writer cannot accept. We do not mean that they would necessarily be of similar composition, but that the theory implies a general similarity of structure and arrangement, so that forces affecting the continental strata in such a way as to throw them into an *upward* curve, at the same time threw those now under the ocean into a *downward* curve. The weak point in this theory, so clearly expounded by Prof. Lapworth, is that we do not know of the existence of anything like groups or series of stratified rocks on the deeper parts of the sea bed, such as we find forming the dry land of continents. According to his teaching, mountain chains, continental arches, and even the deepest oceanic troughs, are simply different parts of great groups of strata, some pushed upwards, some downwards, and some squeezed up into highly-compressed ridges to form mountain chains. Now, the continental arches and the mountain ranges are made up of the same materials, and are brought into position by the same forces; and at first sight one would conclude that the oceanic depressions must be only another phase of the same phenomena. But what if these hollows are primitive depressions on the earth's surface uncovered by strata, save for a thin layer of the "red clay," an abysmal deposit discovered of late years by soundings? In that case the continuity is broken, and the oceanic trough is no longer a continuation of the continental arch. This brings

us to the question of "the permanence of ocean basins," a doctrine that has been much discussed of late years. The more extreme advocates of this theory hold that the great oceanic depressions have, in the main, always been depressions, and therefore below sea-level. while the great land areas or continents have, in the main, been always above sea-level. They only will allow a certain amount of interchange between these two areas along the margins of the continents where soundings are shallow, and a slight elevation would make the sea bed dry land, as, for instance, the North Sea. Without binding ourselves down to this theory, we may admit that there is evidence in its favour, but perhaps it is only true for the deepest oceanic depressions.

No one has yet succeeded in showing why either the mountain chains or the broad folds of the continents and the troughs of the oceans take the directions that have somehow been given to them. The two land masses of the new world, North and South America, trend north and south, as do their mountain ranges. The Euro-Asiatic continent, on the other hand, trends east and west, as does its main axis of elevation, *i.e.*, the great chain of mountains beginning with the Pyrennees and ending with the Atlai Mountains. Why are these things so? This is a problem yet to be solved by the new geology, but at any rate it would seem that the prevailing lines of elevation have been determined at some very early period in the world's history, probably in pre-geological times.

Look at a map of the world, and you will see that the great land masses, and some of the smaller ones too, all point southwards. The most striking examples are North and South America, Greenland, Africa; but the same rule applies to Scandinavia, Spain, Italy, Albania and Greece, Arabia, India, Malacca, and even the two ends of Australia. In some cases—as, for example, Scandinavia and Italy—the reason is obvious, viz., the direction of mountain ranges. But probably several causes have combined to bring about this remarkable result. Perhaps ocean currents may be partly responsible, for in the southern hemisphere there is a decided surface drift northwards, and perhaps the currents wear away the ends of our continents.

Prof. Suess, the leader of the new school of geology in Vienna, questions the usually accepted doctrine that continents are due to elevation, and thus strikes at the root of ideas which, to most geologists, have become axioms. He considers that we have no evidence of any vertical elevation affecting wide areas such as continents, and that the only movements of elevation that take place are those by which mountains are upheaved. Most geologists believe that the sea-level has remained constant, as Lyell taught; but Suess abandons this and says that the water leaves the land, owing to changes in sea-level, instead of the land leaving the water. This is somewhat startling, and it will be interesting to see what effect such teaching will have on English geologists. In equatorial regions, according to Suess, the sea is gaining upon the land, while in other latitudes the reverse would appear to be the case. This is in harmony with his view of a periodic flux and reflux of the waters of the ocean between the equator and the poles. Another foreign geologist, Schmick, believes in grand secular movements of the ocean in order to account for apparent elevation and depression of land areas. These theories are a partial revival and development of certain old-fashioned views which, till lately, had been looked upon as quite out of date. Thus, Celsius, 150 years ago, after examining the coast lines of Sweden, came to the conclusion that the sea had retreated from the land, owing to a gradual drying up of the ocean.

But to return to the present: we find our lands distributed over the surface of a great continental plateau, the edges of which plunge down more or less steeply into the depths of the ocean, where some great depressions exist. Most geologists believe that the continental plateaux and the great oceanic depressions have never changed places. But at the same time shore lines have advanced and retreated many times, so as to bring about geographical changes of more or less magnitude, without causing a complete shifting of these two broad features. Between them there is often a kind of neutral ground which may at one time be dry land, at another, sea. Let us now consider the relation between coast lines and the slopes or margins of the great plateaux. Roughly speaking, our existing coast lines may be said to trend in the same general direction as these margins. It can be shown that there are two very distinct types of coast line. and that they are determined by the extent to which they correspond with the margins of the plateaux. First we have coast lines of a simple type, running for long distances in one general direction, and not broken up into innumerable minor features. Of such a kind are the east and west coast lines of Africa, or those of the greater part of North and South America. Now in these cases the coast lines are near to the steep slopes of the several plateaux, so that a rise of several thousands of feet would make very little change. The second type of coast line is quite different; instead of being regular, it is very irregular, nor does it run mainly in one direction. They are broken up by promontories and peninsulas, by inlets and fiords and islands. For example, we may take the coast line of north-western Europe, or the corresponding latitudes of North America. Here we have regions of comparatively recent depression which has caused the sea to come creeping over what before was land. Such regions are partially submerged lands, the surface features of which contribute to make the irregular coast lines. Thus the fiords of Norway are simply land valleys submerged, while the islands are hills partly submerged; the same applies to the west coast of Scotland. An elevation of north Europe, to the extent of 500 fathoms, would make the sea go back to the margin of the European plateaux, and give us a smooth and regular coast line about 200 miles west of the Irish coast. These irregular coast lines, then, are due to surface features of the continental plateaux, formerly made by atmospheric denudation, namely, hills and valleys. A glance at a good physical map, showing ocean contour lines, will show that they are regular and of unbroken outline, except in those shallow seas that really belong to the continental plateaux.

Finally, the coast lines of the world's continents are of very different ages; some are young, born, as it were, of yesterday, while others are of high geological antiquity. Those of the Atlantic Ocean are very ancient, while those bounding the Pacific are comparatively recent. Now this explains why the latter ocean is bounded by high mountain ranges, and the former only by low hills. These low hills are the remnants of former great mountain ranges worn down to mere stumps by ages of denudation. Thus the hills of Devonshire, Wales, Scotland, and Norway are known to be of very ancient date, and their geological structure tells us that they are mere remnants of what were once mighty ranges of mountains comparable with the Alps or the Himalayas. We also know that formerly volcanic action on a great scale took place along these old ranges; but that has died out, and they have reached a state of stability or equilibrium. Along the margins of the Pacific, however, we have a great circle of active volcanoes acting along mountain ranges of great height,

and only recently upheaved. Time has not yet brought them low by denudation, nor have they reached a state of equilibrium between external and internal forces, so that they still manifest volcanic action. Thus we see the two types of coast line well exemplified, and these may be called the Atlantic and Pacific types.

THE FACE OF THE SKY FOR NOVEMBER.

By HERBERT SADLER, F.R.A.S.

SOLAR spots and faculæ show little diminution either in number or magnitude. The following are conveniently observable minima of two Algol-type variables :—Algol, November 3rd, 11h. 51m. P.M. ; 6th, 8h. 40m. P.M. ; 9th, 5h. 29m. P.M. ; 26th, 10h. 22m. P.M. ; 29th, 7h. 12m. P.M. U Cephei, November 4th, 11h. 20m. P.M. ; 9th 11h. 0m. P.M. ; 14th, 10h. 40m. P.M. ; 19th, 10h. 19m. P.M. ; 24th, 9h. 59m. P.M. ; 29th, 9h. 39m. P.M.

Mercury is an evening star throughout November, but for the first three weeks of the month he is too near the Sun to be seen, and after that his great southern declination will prevent his being satisfactorily observed in these latitudes. He is at his greatest eastern elongation ($22\frac{3}{4}°$) on the 23rd. He is at his brightest about the 30th, when he sets at 4h. 53m. P.M., or one hour after sunset, with a southern declination of $25\frac{1}{2}°$, and an apparent diameter of 8·0″, $\frac{35}{100}$ths of the disc being illuminated. During the last week in the month he describes a short direct path from Ophiuchus into Sagittarius. Venus is a morning star, and though still a conspicuous object in the heavens, is rapidly dwindling in brightness. On the 1st she rises at 2h. 52m. A.M., with a northern declination of 2° 7′, and an apparent diameter of 16″, $\frac{71}{100}$ths of the disc being illuminated, and her brightness being about equal to what it was at the middle of last March. On the 16th she rises at 3h. 36m. A.M., with a southern declination of 4° 26′, and an apparent diameter of $14\frac{1}{2}″$, just three-quarters of the disc being illuminated. On the 30th she rises at 4h. 13m. A.M., with a southern declination of $10\frac{1}{2}°$, and an apparent diameter of $13\frac{1}{2}″$, $\frac{79}{100}$ths of the disc being illuminated, and her brightness being about equal to what it was at the end of February last. During the month she pursues a direct path through Virgo, but without approaching any naked-eye star very closely.

Mars is an evening star, and though his brightness and apparent diameter have notably decreased, he is nevertheless better situated for observation on account of his increasing elevation above the horizon. On the 1st he rises at 2h. 5m. P.M., with a southern declination of 15° 4′, and an apparent diameter of 12·4″, the defect of illumination on the following limb being obvious. His brightness is then but little more than one-fifth of what it was at opposition. On the 16th he sets at 11h. 46m. P.M., with a southern declination of $11\frac{1}{2}°$, and an apparent diameter of 10·8″. On the 30th he sets at 11h. 41m. P.M., with a southern declination of 7° $50\frac{1}{2}′$, and an apparent diameter of 9·6″, the apparent brightness of the planet being only one-eighth of what it was at opposition. During the month Mars describes a direct path through Aquarius, being very near the $4\frac{1}{2}$ magnitude star ι Aquarii on the evenings of the 3rd and 4th.

Ceres is an evening star, being in opposition to the Sun on the 15th, at a distance from the earth of about $166\frac{1}{2}$ millions of miles. She souths on the 20th at 11h. 29m. P.M., with a northern declination of 11° 46′. She will appear as a $7\frac{3}{4}$ magnitude star at the present opposition. During November she pursues a retrograde path through Taurus, but without approaching any naked-eye star. For measures of the diameter of Ceres *cf.* "Face of the Sky" for May, 1890. Vesta is also an evening star, being in opposition to the Sun on the 12th, at a distance from the earth of about 146 millions of miles. On the 20th she souths at 11h. 12m. P.M., with a northern declination of 8° 44′. She will appear as a $6\frac{3}{4}$ magnitude star at the present opposition. During November she pursues a retrograde path through Taurus to the confines of Cetus. At 10h. P.M. on the 8th she will be 6′ due north of the $5\frac{3}{4}$ magnitude star *t* 6 Tauri, and on the evening of the 15th she will be less than $\frac{1}{2}°$ *nf.*, and on the 16th less than $\frac{1}{4}°$ *np.*, the $3\frac{1}{2}$ magnitude star ο Tauri. For measures of the diameter of Vesta *cf.* "Face of the Sky" for January, 1890.

Jupiter is an evening star, and is still by far the most magnificent object in the evening sky. He rises on the 1st at 3h. 24m. P.M., with a northern declination of 5° 22′, and an apparent equatorial diameter of $48\frac{1}{2}″$. On the 30th he rises at 1h. 50m. P.M., with a northern declination of 4° 37′, and an apparent equatorial diameter of $45\frac{1}{4}″$. During the month he describes a retrograde path in Pisces, being about 28′ north of the 6th magnitude star 77 Piscium (a very pretty wide pair) on the 18th, and rather over 30′ south of the 6th magnitude star 73 Piscium on the 22nd. Shortly after midnight on the 3rd a $9\frac{1}{4}$ magnitude star will be $\frac{3}{4}′$ north of the planet, and at $7\frac{1}{4}$h. P.M. on the 24th a $9\frac{1}{2}$ magnitude star will be $\frac{3}{4}′$ south of the planet. The following phenomena of the satellites occur while Jupiter is more than 8° above and the Sun more than 8° below the horizon. On the 2nd a transit ingress of the first satellite at 11h. 2m. P.M. ; of its shadow at 11h. 34m. P.M. ; an occultation disappearance of the second satellite at 11h. 42m. P.M. On the 3rd an occultation disappearance of the first satellite at 8h. 16m. P.M., and its reappearance from eclipse at 11h. 1m. 11s. P.M. On the 4th a transit ingress of the first satellite at 5h. 28m. P.M. ; of its shadow at 6h. 3m. P.M. ; a transit ingress of the second satellite at 6h. 25m. P.M., and of its shadow at 7h. 34m. P.M. ; a transit egress of the first satellite at 7h. 41m. P.M., and of its shadow at 8h. 16m. P.M. ; a transit egress of the second satellite at 8h. 54m. P.M., and of its shadow at 10h. 5m. P.M. On the fifth an eclipse reappearance of the first satellite at 5h. 30m. 7s. P.M. On the 6th a transit egress of the third satellite at 5h. 33m. P.M. ; a transit ingress of its shadow at 5h. 57m. P.M. ; a transit egress of the shadow at 8h. 12m. P.M. On the 10th an occultation disappearance of the first satellite at 10h. 2m. P.M. On the 11th a transit ingress of the first satellite at 7h. 13m. P.M., and of its shadow at 7h. 58m. P.M.; a transit ingress of the second satellite at 8h. 44m. P.M. ; a transit egress of the first satellite at 9h. 26m. P.M., and of its shadow at 10h. 11m. P.M. ; a transit ingress of the shadow of the second satellite at 10h. 13m. P.M., and a transit egress of the second satellite at 11h. 14m. P.M. On the 12th an eclipse reappearance of the first satellite at 7h. 25m. 42s. P.M. On the 13th a transit ingress of the third satellite at 6h. 49m. P.M. ; an eclipse reappearance of the second satellite at 7h. 7m. 4s. P.M. ; a transit egress of the third satellite at 8h. 59m. P.M. ; a transit ingress of the shadow of the third satellite at 9h. 59m. P.M. On the 17th an occultation disappearance of the first satellite at 11h. 49m. P.M. On the 18th a transit ingress of the first satellite at 9h. 0m. P.M. ; and of its shadow at 9h. 53m. P.M. ; a transit ingress of the second satellite at 11h. 5m. P.M. ; a transit egress of the first satellite at 11h. 13m. P.M. On the 19th an occultation disappearance of the first satellite at 6h. 16m. P.M., and its reappearance from eclipse at 9h. 21m. 22s. P.M. On the 20th an occultation disappearance of the second satellite

at 5h. 25m. p.m.; a transit egress of the first satellite at 5h. 40m. p.m., and of its shadow at 6h. 35m. p.m.; an eclipse reappearance of the second satellite at 9h. 42m. 48s. p.m.; a transit ingress of the third satellite at 10h. 15m. p.m. On the 24th an eclipse reappearance of the third satellite at 5h. 59m. 56s. p.m. On the 25th a transit ingress of the first satellite at 10h. 47m. p.m., and of its shadow at 11h. 48m. p.m. On the 26th an occultation disappearance of the first satellite at 8h. 4m. p.m., and its reappearance from eclipse at 11h. 17m. 6s. p.m. On the 27th a transit ingress of the first satellite at 5h. 14m. p.m., and of its shadow at 6h. 17m. p.m.; a transit egress of the first satellite at 7h. 28m. p.m.; an occultation disappearance of the second satellite at 7h. 47m. p.m.; a transit egress of the shadow of the first satellite at 8h. 30m. p.m. On the 28th an eclipse reappearance of the first satellite at 5h. 46m. p.m. On the 29th a transit ingress of the shadow of the second satellite at 4h. 50m. p.m.; a transit egress of the satellite itself at 5h. 12m. p.m., and of its shadow at 7h. 19m. p.m.

Saturn does not rise until 2h. 11m. a.m. on the last day of the month, but it may be mentioned that on the early morning of the 13th he will be little more than the diameter of the Moon south of γ Virginis, the star and planet appearing as a wide double star to the naked eye. Uranus is invisible.

Neptune is admirably placed for observation, rising on the 1st at 5h. 55m. p.m., with a northern declination of 20° 29′, and an apparent diameter of 2·7″. On the 30th he rises at 3h. 58m. p.m., with a northern declination of 20° 22′. During the month he describes a short retrograde path between τ and ϵ Tauri, to the E. N. E. of the $5\frac{3}{4}$ magnitude star Weisse's Bessel², iv.h. 650. A map of the small stars near the path of Neptune from November 1st, 1892, to May 1st, 1893, is given in the *English Mechanic* for October 28th.

November is a very favourable month for shooting stars. The most marked displays are the *Leonids*, on November 13th and 14th, the radiant point being in r.a. 10h. 0m., northern declination + 23°. The radiant point rises at about 10h. 15m. p.m. The *Andromedes* occur on the 27th, the radiant point being in r.a. 1h. 40m., northern declination 43°.

The Moon is full at 3h. 49m. p.m. on the 4th; enters her last quarter at 10h. 2m. a.m. on the 11th; is new at 1h. 19m. p.m. on the 19th; and enters her first quarter at 10h. 28m. a.m. on the 27th. She is in perigee at 3·9h. p.m. on the 4th (distance from the earth 221,630 miles), and in apogee at 5·1h. a.m. on the 18th (distance from the earth 252,680 miles). The greatest eastern libration takes place at 10h. 7m. p.m. on the 10th, and the greatest western at 1h. 31m. a.m. on the 27th. There will be a total eclipse of the Moon on November 4th, partly visible as a partial eclipse at Greenwich. The first contact with the penumbra is at 1h. 11·6m. p.m.; with the shadow (at 91° from the northernmost point of the Moon's limb towards the east, direct image) at 2h. 9·2m. p.m.; beginning of total phase, 3h. 23·0m. p.m.; middle of the eclipse, 3h. 45·0m. p.m.; end of total phase, 4h. 7·0m. p.m.; last contact with shadow (at 138° from the northernmost point of the Moon's limb towards the west, direct image), 5h. 20·8m. p.m.; last contact with the penumbra, 6h. 18·4m. p.m. The magnitude of the eclipse (Moon's diameter = 1), 1·092. At Greenwich the Moon does not rise till 4h. 21m. p.m., and the Sun sets at 4h. 25m. p.m. This is rather a remarkable eclipse, as the Moon attains her perigee within ten minutes of her being full and of the middle of the eclipse, and her approach to the earth is exceptionally close, the minimum distance at perigee being, according to Neison, 221,614 miles.

Chess Column.

By C. D. Locock, B.A.Oxon.

All communications for this column should be addressed to the "Chess Editor, *Knowledge Office*," and posted before the 10th of each month.

Solution of October Problem (by G. K. Ansell):—

1. Q to Q5; followed by 2. Q to R8, 2. Q × B, 2. Kt × B, 2. P × B, or 2. Q to Q4ch, according to Black's play.

Correct Solutions received from G. S. Cummings, H. S. Brandreth, and F. O. Lane.

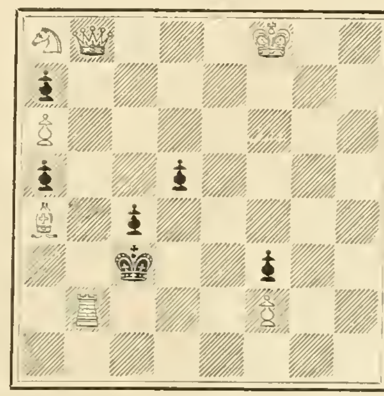
Max v. Pochhammer.—The published solution of the September problem was quite correct. What is your difficulty?

H. S. Brandreth.—Taking your question as referring to a possible solution tournament, I regret to say that the matter is not yet arranged; will give details when possible.

F. O. Lane.—Your analysis is not quite exhaustive, as you will see above.

PROBLEM.

Black.



White.

White to play, and mate in three moves.

[This problem appeared some years ago in a French column, and seems worthy of resurrection. Mr. Steinitz admired it, and copied it in his own magazine. The composer's name has escaped our memory.]

The following is one of the most notable games played by the winner in the late Dresden tournament:—

Bishops' Gambit.

White (Mr. Winawer).	Black (Dr. Tarrasch).
1. P to K4	1. P to K4
2. P to KB4	2. P × P
3. B to B4	3. P to Q4
4. B × P	4. Q to R5ch
5. K to Bsq	5. P to KKt4
6. KKt to B3 (*a*)	6. Q to R4
7. P to KR4	7. B to Kt2 (*b*)
8. P to Q4	8. Kt to K2
9. Kt to B3	9. P to KR3
10. K to Ktsq	10. Q to Kt3 (*c*)
11. Q to Q3	11. P to QB3
12. P to R5 (*d*)	12. Q to R2
13. B to Kt3	13. Castles
14. Kt to K2	14. B to Kt5
15. B to Q2	15. Kt to Q2
16. QB × P (*e*)	16. P × B
17. Kt × P	17. B × Kt (*f*)
18. P × B	18. QR to Qsq

19. Q to K3 (*g*)	19. Kt to QB4 !
20. P to B3	20. Kt × B
21. P × Kt	21. P to R3 (*h*)
22. KR to R2	22. KR to Ksq
23. Kt to K2 (*i*)	23. Kt to Q4 (*j*)
24. Q to B2	24. Kt to B2
25. Kt to Kt3	25. Kt to K3
26. Kt to B5	26. Kt to Kt4
27. K to Rsq	27. K to Rsq
28. R to Kt2 (*k*)	28. B to B3
29. Kt to K3 (*l*)	29. Kt to K3
30. R to Kt4	30. B to Kt4
31. Kt to B5	31. R to KKtsq
32. Q to R2	32. B to B3
33. QR to KKtsq	33. R to Kt4 (*m*)
34. P to KB4	34. R × Kt
35. P × R	35. Q × P
36. R(Kt)sq to Kt2 (*n*)	36. Kt to Kt2
37. R(Kt4) to Kt3	37. Q to Kt8ch (*o*)
38. RKtsq	38. Q to K5ch
39. R(Ktsq) to Kt2	39. Q to Kt8ch
40. R to Ktsq	40. Q to K5ch
41. R(Kt3) to Kt2 (*p*)	41. R to Q4 !
42. P to KB5	42. R × BP
43. Q to Kt8ch	43. K to R2
44. R to Qsq	44. R × Pch
45. K to Ktsq	45. Q to K6ch
46. R to B2	46. B to R5
Resigns.	

Notes.

(*a*) White develops his pieces in the old-fashioned order. 6. QKt to B3, and 7. P to Q4, usually precede this. The position becomes normal at the tenth move.

(*b*) Best. If 7. . . P to KR3? White may play 8. B × Pch, Q × B ; 9. Kt to K5, etc.

(*c*) Considered stronger than the old continuation 10. . . . P to Kt5 ; 11. Kt to Ksq ! P to B6 ; 12. P × P, or 12. B to K3 with a good game.

(*d*) Though this drives the Black Queen out of play for some time, it secures Black's advantage on the King's side, and is on that account inadvisable.

(*e*) If at all, why not on his previous move? But the delay is characteristic of Winawer's sometimes incomprehensible style, *vide* his extraordinary Ruy Lopez beginning, 1. P to K4, P to K4 ; 2. KKt to B3, QKt to B3 ; 3. B to Kt5, P to QR3 ; 4. B to R4, Kt to B3 ; 5. B × Kt (! !)—a unique instance. Instead of the unsound sacrifice in the text he should advance the QBP.

(*f*) Instead of this exchange we should prefer 17. . . QR to Qsq, threatening 18. QKt to B4. This would have adequately forestalled the threatened 18. Kt to R2.

(*g*) As this does not prevent Kt to QB4, P to B3 at once is perhaps better, and would at least give the White Queen the choice of other squares when attacked.

(*h*) Loss of time, as the Pawn cannot well be taken, *e.g.*, 21. . . KR to Ksq ; 22. R × P? Kt to B4 ; 23. Q moves, Kt × QP, etc.

(*i*) Mr. Winawer now develops an ingenious attack. The text move is also partly defensive, to guard against the sacrifice pointed out in the previous note.

(*j*) Against the opposing array of Pawns Black's game is not easy. Dr. Tarrasch forms the admirable plan of manœuvring the Knight to KKt4, though he subsequently retires it again in favour of the Bishop.

(*k*) Better than taking the open file with the other Rook, which might conceivably be wanted elsewhere. He now threatens Kt × B and P to KB4.

(*l*) Waste of time, as his 31st move shows. 29. R to Kt4 seems preferable.

(*m*) An excellent move. The Rook exerts pressure in all directions. The sacrifice of the exchange next move is probably the quickest method of winning, and is at any rate justified by the result.

(*n*) For Black threatened, among other things, to win a Pawn by . . . Q to Q4ch. On his next move he finds himself with nothing to do.

(*o*) Black indulges in a series of checks in order to gain time. Q × BP would only complicate the game unnecessarily.

(*p*) A disastrous change of plan. This Rook is required for R3, and its removal terminates the game.

CHESS INTELLIGENCE.

The final score in the Belfast quadrangular tournament was as follows :—J. H. Blackburne, 5½, and J. Mason, 5½, bracketed first ; H. E. Bird, 5, and F. J. Lee, 2. Mr. Bird was signally successful against Mr. Blackburne. Otherwise the results were in accordance with time-honoured precedents. In such company Mr. Mason is almost invariably a safe second. Mr. Lasker's absence was a matter for regret, but it guaranteed his arrival in America with a clean record.

Mr. T. H. Moore, the Hon. Secretary of the Ludgate Circus Chess Club, is endeavouring to form a Metropolitan Chess Association, and has convened a meeting of chess-players to consider his scheme. Having regard to the very large number of Chess clubs in London, the object seems a desirable one, and it is surprising that no one was found willing to undertake the task before.

A match of two games, by correspondence, played recently between the Liverpool and Glasgow Clubs, resulted in a victory for the former club, who won one game (Irregular) and drew the other (Ruy Lopez). The Liverpool players had the assistance of Mr. Burn in the earlier stages of the contest only.

Owing mainly to the efforts of Messrs. I. M. Brown and L. P. Rees, the long-discussed match between the North and South of England has now been definitely arranged. The match will take place at Birmingham early next year, probably with fifty players a side. Messrs. Owen and Wayte will captain the teams.

The annual winter tournament of the City of London Chess Club has just been started on the usual gigantic scale. There are 144 entries, and the contest may be expected to outlast the winter.

Contents of No. 84.

TERMS OF SUBSCRIPTION.

"Knowledge" as a Monthly Magazine cannot be registered as a Newspaper for transmission abroad. The Terms of Subscription per annum are therefore as follows :—To any address in the United Kingdom, the Continent, Canada, United States, Egypt, India, and other places in the Postal Union, the Subscription is 6 shillings, including postage ; or 1 dollar 50 cents ; or 6 marks ; or 7 francs 50 cents.

For all places outside the Postal Union, 5 shillings in addition to the postage.

Communications for the Editor and Books for Review should be addressed Editor, "Knowledge" Office, 326, High Holborn, W.C.

KNOWLEDGE

AN ILLUSTRATED

MAGAZINE OF SCIENCE

SIMPLY WORDED—EXACTLY DESCRIBED

LONDON: DECEMBER 1, 1892.

CONTENTS.

ARMADILLOS AND AARD-VARKS.

By R. LYDEKKER, B.A.Cantab.

OF the three animals represented in the figures accompanying the present article, two are sufficiently alike to suggest to the ordinary observer their relationship to one another, but the third is so utterly different, that it is difficult to point out any important character it has in common with the two others; nevertheless, naturalists generally regard all these three strange creatures as belonging to a single order of mammals, for which the name of Edentata is adopted.

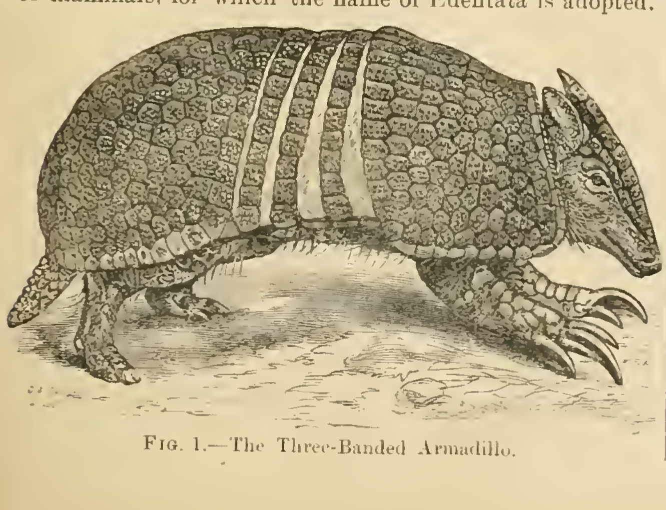


FIG. 1.—The Three-Banded Armadillo.

The signification of the term Edentata being toothless, the unsophisticated student would naturally be led to suppose that all the animals so named were utterly devoid of those useful but troublesome appendages. This, however, is far from being the case, the majority of the members of the group (among which are those figured here) having a considerable number of teeth. Still there is one feature in connection with the dentition exhibited by the whole of these so-called edentates; and this is, that teeth in the front of the jaws, corresponding to the incisors of other mammals, are totally absent. Instead, therefore, of being described as edentates, or toothless mammals, these creatures ought rather to have been named aprotodonts, or incisorless mammals. After all, however, it is not much consequence what is the proper meaning of a name, so long as we know the sense in which it is used, and there is accordingly no real objection to the employment of the term edentates, which has obtained the almost universal sanction of zoologists.

In addition to this total absence of front teeth, the edentates are further characterized by the circumstance that all their teeth (when they possess any) show no trace of the hard layer of enamel which is so characteristic and essential a constituent of those of other mammals; these teeth at no period of life forming roots, but continually growing from below. Moreover, in nearly all the edentates there is never any set of milk-teeth developed, although, unfortunately, this cannot be taken as a characteristic of the order, since such teeth occur in one of the armadillos, and also in the animal represented in our third figure.

Premising that the edentates are quite distinct from the marsupials and egg-laying mammals, we may say, then, that the only features by which they can be collectively characterized are the want of front teeth, and the absence of enamel on those of the cheek-series, while in certain rare instances they may be utterly devoid of teeth. Such characters, it must be confessed, are by no means of first importance.

The mammals thus associated by these negative characteristics are now chiefly confined to the southern hemisphere, and include the sloths, anteaters, and armadillos of South America, the pangolins, or scaly anteaters of South-eastern Asia and Africa, and the aard-varks of Africa; the true anteaters and pangolins being those in which teeth are wanting. In past times they were also represented by the gigantic megathere, and a number of other allied extinct forms ranging throughout America, which in some respects serve to connect the sloths with the anteaters. This marked restriction of the existing edentates to the southern hemisphere, and their especial abundance in South America, at once stamps them as a very lowly group of animals, there being a well-marked tendency for the preservation of the humbler forms of life in the southern continents and islands of the globe. There is, indeed, a question whether the pangolins, and more especially the aard-varks of the Old World, have any real kinship with the more typical American edentates, but apart from this possibility of the artificial nature of the group as at present constituted there can be no doubt but that all its members are what may be called degraded types—that is to say, that instead of having advanced in the struggle for existence they have lost some of the attributes of the higher animals; evidence of this degradation being afforded by the indications above mentioned of their having been formerly provided with two sets of teeth. In saying that the edentates are lowly and degraded examples of the mammalian type we by no means intend, however, to imply that they are not admirably adapted to

their own particular modes of life, or that they have not developed special structures unknown among the higher mammals. In truth, precisely the contrary is the case, since these creatures have taken to modes of life unlike those of the majority of the larger mammals, and have more or less specially modified their structure in accordance with such peculiar habits, so that they are thoroughly and perfectly suited to their environment. And we may add that it is doubtless due to these peculiarities of structure and habits that they have been enabled to survive and hold their own against the competition of the higher forms.

Thus, in the first place, with the exception of a few armadillos, the whole of the edentates are strictly nocturnals; and while the sloths spend the whole of their lives among the branches of the dense forests of South America, all the others have taken to a burrowing subterranean life. Moreover, as we have noticed in an earlier article on "Mail-Clad Animals," the armadillos and the pangolins have acquired a special protection from their foes in the shape of a bony or scaly armour, which is a perfectly unique feature in the whole mammalian class. Another peculiarity of the group is that no less than three distinct sections of its members—namely, the anteaters of South America, the pangolins of Southern Asia and Africa, and the African aard-varks—have taken to feed mainly or exclusively on termites, or so-called white ants. This practice obviously gives them an advantage in the struggle of existence, since, with the exception of the marsupial banded anteater and the egg-laying spiny anteater of Australia (with which, of course, they do not come into competition), no other mammals are in the habit of subsisting exclusively on those insects. And we may notice here that of the three groups of termite-eating edentates, two—namely, the pangolins and the anteaters—are those which have entirely lost their teeth; while in the aard-varks those organs are retained. As teeth are obviously of no sort of use to animals subsisting on such a diet, we may regard the two former groups as those most specially modified for their particular mode of existence; and it may thus be suggested that they have taken to termite-eating for a longer period than the aard-varks. A similar observation also applies to the banded and spiny anteaters of the antipodes, the former retaining a number of minute teeth, while in the latter they have completely disappeared. Needless to say, all termite-eating mammals, whether they be edentates, marsupials, or egg-layers, have extremely long, narrow, and extensile tongues with which to pick up their insect-food; but the presence of such an organ does not, of course, imply any mutual affinities between the possessors thereof, and is merely an instance of the similarity of organs arising from adaptation to a similar mode of existence. The tongue of the aard-varks is, however, far less elongated and extensile than that of the pangolins and true anteaters; and, therefore, tends to confirm our suggestion as to the relative duration of time since the ancestors of these creatures severally took to termite-eating. Another instance of adaptation displayed by all the edentates, except the arboreal sloths, is to be found in the powerful and generally elongated claws or nails with which their feet are armed, such claws being obviously necessary for a fossorial subterranean existence. The aard-varks, as will be seen from our third figure, have, however, much shorter and blunter claws than any other member of the group; and this leads me to hazard the suggestion that, in addition to having taken at a comparatively late period to termite-eating, these animals have not been accustomed to a subterranean life for so long a time as their reputed kindred.

Having said thus much as to edentates in general, we must turn to the special consideration of the creatures whose names form the title of this article.

The armadillos, as their name (a Spanish one) implies, are distinguished by the solid armour with which their heads and backs are protected; and it is doubtless the peculiar appearance presented by these animals to which we owe the expression "hog-in-armour." In all the armadillo family the armour takes the form of a series of thicker or thinner bony plates embedded in the skin covering the head and back, and overlain by horny scales; while the under parts of the body and limbs are hairy, and in many species a larger or smaller number of stiff hairs protrude from between the joints of the armour. This bony armour is a perfectly unique feature among existing mammals; and since each plate is ornamented with a more or less elaborate sculptured pattern, such armour when cleaned by maceration forms a most beautiful object. In the true armadillos, as the one represented in Fig. 1, the shield of armour covering the head is quite distinct from that of the body; while the latter is divided into three distinct portions, namely, a large solid shield covering the fore-quarters, and separated by a larger or smaller number of free movable bands occupying the middle of the body from a nearly

Fig. 2.—The Pichiciago. (From Jardine.)

similar shield protecting the hinder portion of the animal. In our figured example the number of the movable bands is only three, but they may vary from six to nine up to as many as twelve or thirteen in other species. In one extinct armadillo there were, however, no solid shields, the whole body being covered by a series of thirty-two movable bands. The latter species evidently, therefore, leads on to the rare and beautiful little creature represented in our second illustration, which rejoices in the name of pichiciago. In this tiny animal, which is only about five inches in length, and has a pink-coloured armour above, and long silky white hair below, the armour of the head and body forms a continuous shield of horny plates underlain by very thin plates of bone, and is attached only to the middle line of the back, so that the lateral portions form a kind of cloak loosely overhanging the hairy sides of the body. The hinder end of this cloak is abruptly truncated, and beneath it the hind-quarters of the animal are protected by a solid bony shield, through a hole in the centre of which protrudes the small cylindrical tail. When the animal creeps beneath a crevice in rocks, as shown in the right hand corner of our illustration, which is not sufficient to conceal its whole body, the strong shield on the quarters affords an ample protection against all attacks. The pichiciago is found on sandy plains only in the western portions of the Argentine pampas. It will be seen from our illustrations that this creature also differs from the true armadillos in the absence of the large external ears which form such a characteristic feature in the physiognomy of the latter.

Reverting to the true armadillos, we find that the majority of the species protect themselves from attack by squatting on the ground, and tucking their limbs within the shelter of the edges of the armour of the body, while the plated head is drawn as close as possible to the front shield. On the other hand, the species represented in our illustration has the power of rolling itself up into a complete ball, like the pill-millipedes of our own country, the wedge-shaped head and tail fitting most perfectly side by side into the deep notches of the front and hind shields. Thus coiled up, the three-banded armadillo is safe from most animals except man. Trusting in this immunity from attack, this armadillo, together with two other species inhabiting the Argentine, has become almost exclusively diurnal in its habits. These diurnal habits, as Mr. W. H. Hudson, in his charming work, "The Naturalist in La Plata," suggests, may also have had the advantage of avoiding any encounters with the larger animals of prey, which are mostly nocturnal, and some of which may have been able to break through the protecting armour, more especially in the species which lack the power of rolling themselves up. Whatever advantage may have formerly accrued from these diurnal habits before the appearance of man on the scene, is, however, now completely lost in cultivated districts, where these species stand a good chance of being completely exterminated by the hand of man.

On the other hand, the six-banded pelado, or hairy armadillo, of the Argentine, which differs from its cousins in preferring an omnivorous diet to one of insects, is a far wiser beast in its generation. This creature, according to Mr. Hudson, adapts itself to the conditions under which it exists, and thus stands a good chance of surviving when its fully-armoured relatives perish. "Where nocturnal carnivores are its enemies," writes the observer mentioned, "it is diurnal; but where man appears as a chief persecutor, it becomes nocturnal. It is much hunted for its flesh, dogs being trained for the purpose; yet it actually becomes more abundant as population increases in any district." Another writer says that beneath any decomposing carcase lying in the Argentine pampas, the burrow of a pelado is almost sure to be found; and it is not a little remarkable that the flesh of a creature which has such unpleasant tastes in the matter of diet, should be so eagerly sought after as an article of human consumption.

Before taking leave of the pelado we must not omit to mention two other peculiar habits which are recorded of it by Mr. Hudson, since these also mark it as a creature far above the generality of its kind in point of intelligence. The first of these peculiarities is the ingenious way the creature catches mice, by approaching them with extreme caution, raising itself on its hind-quarters, and then suddenly proceeding to "sit down" on the unfortunate rodents, which become entrapped under the projecting edges of its armour. The sharp edges of the armour are also brought into requisition when this armadillo attacks a snake preparatory to devouring it; the snake being pressed close to the ground beneath the edges of the bony plates, and literally sawn to death by means of a backwards-and-forwards motion of the body of its assailant.

The largest of living armadillos is one which inhabits the moist forests of Brazil and Surinam, and has a length of about 36 inches, exclusive of the unusually long tail, which is some 20 inches in length. These dimensions were, however, vastly exceeded by some extinct armadillo-like animals, of which the remains are found in the caverns of Brazil. The most gigantic of these creatures, which flourished during the pleistocene epoch—the period *par excellence* of giant mammals—is estimated to have been nearly equal in size to a rhinoceros, and has been named the chlamydothere. The armour appears to have been very like that of the true armadillos, but the bony plates measured as much as five and six inches in length, in place of little more than an inch. The teeth differed, however, from the simple conical ones of the modern armadillos, and more nearly resembled the vertically fluted ones characteristic of the extinct glyptodonts. Unfortunately, space does not admit of further reference to the gigantic creatures from the pleistocene of South America, to which the latter name has been applied, all of which are distinguished from the armadillos by the armour of the body being welded into a single solid dome-like shell, of which a specimen is figured in the article on "Mail-Clad Animals."

Passing on to the animals whose name comes second in the title of this article, we have first of all to mention that the designations by which these creatures are commonly known exhibit that remarkable want of originality in nomenclature which appears to be characteristic of Europeans when they are brought for the first time into contact with hitherto unknown animals. Thus, whereas the Dutch Boers of South Africa applied to the creatures in question the title of "aard-vark" (meaning "earth-pig"), the English colonists of the Cape commonly speak of them as the ant-bear. Now, if there is any one particular animal which the aard-vark (as we must perforce term the creature) is unlike, it is a bear; while its resemblance to a pig is only of the most distant kind. Still, however, as in the case of the order to which it belongs, we must be content to designate the animal by the name by which it is most commonly known.

In appearance, aard-varks, of which there are two species, are decidedly ugly creatures, having thick ungainly bodies, a long pointed snout, enormous erect ears, and a thick cylindrical and tapering tail, nearly as long as the body. The skin is either almost naked, or thinly covered with bristle-like hairs. The fore feet have but five toes, which are armed with broad and strong nails, as

are the five toes of the hind limb. As we have already mentioned, almost the only feature which the aard-vark has in common with the armadillos is the absence of front

Fig. 3.—The Ethiopian Aard-Vark. (From Sclater, *Proc. Zool. Soc.*)

teeth, and its cheek-teeth are quite unlike the simple ones of the latter, as, indeed, they are dissimilar to those of any other mammals. In the first place, they are preceded by a functionless series of milk-teeth (a feature found elsewhere among edentates only in one species of armadillo), while in the second place the premolars are unlike the molars. The latter are composed of a number of closely packed denticules, each furnished with a central pulp cavity, and by their close approximation forming polygonal prisms, so that a cross-section of one of these teeth presents the appearance of a pavement. No dental structure among mammals is at all comparable to this, although there is some approximation to it among certain fishes.

Of the two living species of aard-vark, one is confined to South Africa, while the other (represented in our figure) inhabits part of Egypt and other districts in the north-western portion of the same continent. A third species occurs fossil in the pliocene deposits of the Isle of Samos, but with this exception the palæontological record is silent as to the past history of these strange creatures, as to whose origin and relationship to the other animals we are at present utterly in the dark. Indeed, the aard-vark is placed among the edentate mammals chiefly because zoologists do not know where else to put it, and they take that group as a kind of refuge for the destitute. Were it not that the burdening of zoological science with new names is from all points of view to be deprecated, there is, indeed, much justification for regarding these animals as the sole representatives of a distinct order, but, although in some ways such a new departure would be convenient, I do not know that in others it would be of any great advantage. But in including them provisionally among the edentates we have to recollect that their affinities with other members of that group—not even excepting the pangolins—must be extremely remote.

Aard-varks lead what would seem to us a very dull and monotonous kind of life, passing the whole of the day curled up in their deep burrows, which are generally excavated hard by the tall pyramidal hills made by the termites, and only issuing forth at night to dig in the mounds for their favourite insect-food. Not a great many years ago it used to be said at the Cape that wherever a clump of termite hills was to be seen there an aard-vark's burrow might be pretty confidently expected. Unfortunately, however, as we learn from a recent report of the Agricultural Department of the Cape Colony, this is no longer the case, and the aard-vark of that district runs a good chance of being exterminated at no very distant date.

This deplorable result is being brought about by the incessant pursuit of these animals by the natives for the sake of their hides and flesh, and also to their being dug out by Europeans for so-called sport. Their flesh is said to be excellent, and is compared to superior pork; while the value of each hide is about fifteen shillings. This threatened extermination is a very short-sighted policy on the part of the South African farmers, to whom the aard-vark (as the report before us points out) is a valuable ally, not only on account of the enormous number of termites it consumes, but likewise from the circumstance that while it is engaged in digging for these insect-pests it covers with loose earth a quantity of the seeds of grass and other pastoral herbage which would otherwise perish during the hot season. Although there is no likelihood at present of the Ethiopian aard-vark sharing the threatened fate of its southern cousin, yet the extermination of the latter would be a sad loss to zoological science, and we therefore wish every success to a movement which we hear has been set going by the Cape Farmers' Association for the protection of this most strange and curious creature ere it be too late.

ON THE DISTRIBUTION OF STARS IN THE MILKY WAY.

By John Richard Sutton, B.A.Cantab.

SEVERAL considerations seem to point to the conclusion that the galaxy must be generally of a roughly circular section; for if we imagine the true depth of the galaxy along the line of sight to be considerably greater than its true breadth (a form to which Sir John Herschel inclined after his study of the stars in the southern hemisphere), we should have, since the sides of such a figure would probably not be smooth, but would be crowded with excrescences and streamers, a great degree of brightness along the central line of the Milky Way throughout its whole length, and a rapid shading off to the edges; and the greater the depth of the Milky Way along the line of sight, the more would this aspect be exaggerated. On the other hand, if the galaxy be of roughly circular section, the decrease of brightness would be much more gradual—which is indeed the case.

Perhaps Figs. 1 and 2 will make this clearer. Let S be the sun; SA, SB, SC, SD, lines in a plane through the sun at right angles to the direction of the galaxy G; a, b, c, streamers. We see that in Fig. 1 the brightness along SA and SD would be considerably less than along SC, which again would be considerably less than that

along SB.* In Fig. 2, however, the corresponding effect would be much less marked. To these considerations may

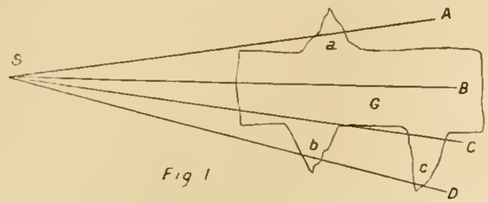


Fig 1

be added that pointed out by the late Mr. Proctor, that the galactic coal-sacks and lacunæ are altogether against any great depth along the line of sight.

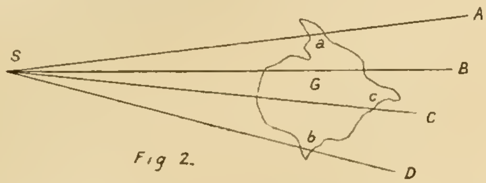


Fig 2.

It seems to me that the strength of this argument (such as it is) gives us something more than a clue to where the greatest proportion of the mass of the Milky Way may be found.

Let us, at a venture, in the absence of any trustworthy evidence one way or the other, make a similar assumption to Sir William Herschel's, and assume the stars to be uniformly distributed throughout the whole Milky Way. Let O P, O Q (Fig. 3) be lines of sight bounding any

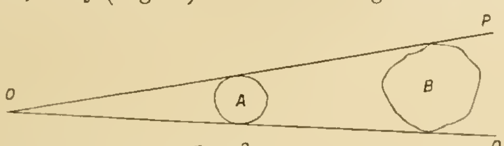


Fig 3

galactic area. The actual distance of this area is unknown, it may be at A or at B, or since the average angular breadth of the galaxy is nearly constant everywhere, let us assume that A is a portion of it from one part of the sky at a certain distance, and that B is a portion from another part of the sky at another distance. A and B are of the same angular dimensions. We want to compare the brightness of A and B on the assumption that each contains the same number of stars in a given volume. Let d and D be the true distances of A and B, δ and Δ their diameters; then

$$\begin{aligned}\text{Volume of A : volume of B} &= \delta^3 : \Delta^3 \\ &= d^3 : D^3 \\ &= d^3 : n^3 d^3 \\ &= 1 \ : n^3\end{aligned}$$

where $D = n\,d$.

But the brightness of each star in B will be reduced in the proportion of $D^2 : d^2$, that is as $n^2 d^2 : d^2$, that is as $n^2 : 1$. Therefore the brilliancy of B will be to that of A in the proportion of $n : 1$. In other words, on the assumption of uniformity of distribution, the brightness of the different parts of the Milky Way would vary as the distance. It has been shown (see KNOWLEDGE for February, 1891) that the brilliancy of a given sheet or volume of stars will remain constant whatever its distance; and this fact has been used to controvert Proctor's spiral theory of the Milky Way. For the main argument for the spiral theory was based upon the erroneous conclusion that the faintness of the great branching streams from the Milky Way was due to an approach to "evanescence through vastness of distance." But on the assumption of a uniformity of distribution, it would be necessary (since the angular breadth of the great branching streams is not materially less than that of the main stream) that these streams, if part of a spiral, should be considerably brighter than the rest of the galaxy,† unless, indeed, the distribution decreased with distance from the sun: an arrangement not altogether impossible perhaps, if we could regard the sun as in some way the mainspring of the whole visible universe.

It will be noticed, however, that the calculation above involves, in fact, Sir William Herschel's other and later hypothesis of a general equality of real size among the stars. If the Milky Way stars are collected into groups of different orders, *i.e.*, if stars of one size segregate into one region, those of another size into another region—a tendency which seems to obtain, to some degree, among the sporadic stars in various parts of the sky—a considerable modification must be introduced into our result. Consider two stars S and s, of masses m and 1, and diameters Δ and δ respectively, at equal distances from the sun. Let their brilliancy, surface for surface, be the same. Then we have—

The total brightness of S : the total brightness of s as the surface of S : surface of s, that is as $\Delta^2 : \delta^2$, but $m : 1 = \Delta^3 : \delta^3$ if the densities are the same.

Therefore $\Delta^2 : \delta^2 = m^{\frac{2}{3}} : 1$.

Therefore the total brightness of S : the total brightness of s as $m^{\frac{2}{3}} : 1$.

If now we could suppose the *mass* of any given volume of the Milky Way to be constant, for every star (S) in the one place we should have m stars (s) in the other place. Hence—

$$\begin{aligned}\text{The total brightness of S : the total brightness of } m \text{ s} &= m^{\frac{2}{3}} : m \\ &= 1 \ : m^{\frac{1}{3}}\end{aligned}$$

so that, on the assumption of equal surface brilliancy, the region containing the smaller stars would be brighter in the ratio $m^{\frac{1}{3}}$ to unity, and it follows that the ratio of the brightness of A to the brightness of B (referring again to the notation of Fig. 3) will be as unity is to $n\,m^{\frac{1}{3}}$, or as $m^{\frac{1}{3}} : n$, according as the smaller stars are found in B or A. In the latter case, if m be greater than n^3, A would be brighter than B. If, then, the galactic stars collected rigorously in classes of magnitude, it would be clear that the brightness, taken alone, of the Milky Way, would have no immediate significance in the present state of our knowledge.

That the galactic stars do collect into minor sheets and volumes of one particular star-magnitude, may be admitted without prejudicing the fact that over any extended region such a tendency is not obvious. Individuality, so to speak, becomes lost in the crowd. Photography adds its testimony to telescopic observation, that the average magnitude of the stars belonging to the Milky Way proper, in any given region, will be about equal to the average star-magnitude of the whole ring. That is to say, if we take a

* Nor would the optical effect be materially different on the view that the Milky Way is a collection of clouds, one behind the other, for a vast distance along the line of sight. I do not contend that it has not this particular cloud structure; what seems certain is, that if it has, the clouds can neither be far apart nor numerous, nor of great individual depth. But see Mr. Ranyard's article in KNOWLEDGE for July, 1890.—J. R. SUTTON.

[† This ingenious reasoning of Mr. Sutton leaves out of account the absorption of light in its transmission to us from distant parts of space. It seems very improbable that the loss of light due to absorption should just balance the increase of brightness of the stream due to its greater thickness, especially when great distances are involved, for the absorption would increase according to an exponential law, while the brightness would, according to Mr. Sutton's assumptions, only increase directly as the distance.—A. C. RANYARD.]

large area of the Milky Way, and divide all the stars we are able to detect in it with a given power, into half a dozen classes of an ascending order of magnitude, containing, say, a, b, c, d, e, f stars respectively in each class, then the ratio $a : b : c : d : e : f$, is nearly enough constant for the same magnitudes over the whole Milky Way.

We have, then, some reason for the inference that we may reason upon the Milky Way stars as upon an equal number of stars of a standard size equal to the average magnitude of the true Milky Way stars. In short, if the Milky Way have a stars giving p units of light each, b stars giving q units of light each, c stars giving r units of light each, &c., we may for present purposes regard it as consisting of $a+b+c+ \ldots$ equal stars each shining with $\frac{(ap+bq+cr+ \ldots)}{(a+b+c+ \;)}$ units of light, and this fraction may be regarded as the standard star for any galactic area of considerable extent, leaving the lucid stars, of course, out of consideration.[*] May we, therefore, argue with equal confidence for an equality of distribution? Clearly we may not, unless we are to adopt the unscientific conclusion to which the first calculation in this paper would lead us that exactly where the Milky Way is brightest there it is farthest away, and that its breadth increases in exactly the same proportion. The only apparent alternative is that the greater brilliancy of the Milky Way in parts of Cygnus and Scorpio, and in the great bright galactic clouds of the southern hemisphere, is due to a greater number of stars in a given volume.

Such evidence as it is amounts to (1) a general circularity of section; (2) a general equality of magnitude; and (3) an unequal distribution of the stars throughout the Milky Way. We can state, therefore, as at least an approximate truth, that the brightness of any part of the Milky Way is a measure of the relative number of its constituent stars, and, in a less degree, of its relative mass. Now the Milky Way in the southern hemisphere is of considerably greater brilliancy than it is in the northern hemisphere; or, to be more correct, the portion of the stream reaching from Cygnus through Aquilla to Argo, is brighter than the remaining portion, an arrangement followed, strangely enough, by the belt of great stars which intersects the Milky Way in Cygnus and Argo. It is in the southern hemisphere, then, that the matter of which the Milky Way is composed should be found in the greatest profusion.

The actual proof or disproof of this purely speculative result, however, can only be gradually evolved by years of work in the observatory. There are two chief difficulties to face: one the difference of texture, so to speak, between the northern Milky Way and the southern; the other, the possibility that the brightness of the galactic clouds may be due in part to local collections of clashing meteoric matter, though it must be admitted that meteoric matter is on the whole quite as likely to stop light as to send it. We can at any rate assert with some confidence that the brighter the region the greater the number of stellar points comprised within it, whether our instruments reveal them individually or not.

[Mr. Sutton leaves out of account brightness due to nebulosity between the stars.—A. C. RANYARD.]

[*] I assume here that the sun is not far from the centre of the galactic ring. The aspect of the Taurus-Orion belt of great stars seems to indicate that the true Milky Way stars are limited to telescope magnitudes. Nearly every bright star on the Milky Way appears to belong in some way to this belt, and the galaxy itself is rich in bright stars because it meets the belt at a very acute angle. This consideration eliminates the lucid stars from consideration here, and hence reduces the number of magnitudes we have to deal with. The exactitude with which the standard star represents the true average star is correspondingly increased.—J. R. SUTTON.

CATERPILLARS.—II.

By E. A. BUTLER.

(*Continued from page* 206.)

CONTINUING the account of caterpillar structure which we commenced last month, we come to the three segments immediately behind the head. Each of these carries a pair of conical legs terminating in a sharp curved claw (Fig. 3). From their situation it is evident they correspond to the legs of the perfect insect, and that the three segments to which they are attached represent the thorax of the butterfly or moth, although there is here no definite line of division, as there would be in the perfect insect, to separate them from the rest of the body. These legs are of considerable use to the caterpillar, not only in walking, but also and more especially in holding the leaves while it is feeding. The blade of the leaf, being attacked at its edge, is steadied between the caterpillar's legs, while its head is stretched out to the full, and its mandibles, having been opened laterally, are brought with a sudden snap down upon the leaf edge lying between them. By this action a fragment is punched out as it were and immediately swallowed, while in far less time than it takes to describe the action, the head is bent round along the arc of a circle and a fresh snip taken as it is brought into each new position, so that in the end a curved excavation is made in the tissues of the leaf.

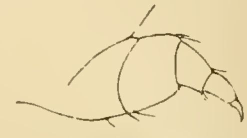


FIG. 3. Leg of Caterpillar of Goat Moth, magnified 8 diameters.

Throughout the whole tribe of caterpillars there are scarcely any other organs that are so uniform in structure as these legs, and there is rarely anything exceptional about them, however varied may be the habits of the insect. But we have one species, the caterpillar of the lobster moth (*Stauropus fagi*), which deviates notably from this uniformity—a deviation sufficiently remarkable to lead us to suspect and look about for some adequate cause. In this rare and curious caterpillar two of the three pairs of legs are greatly elongated and, pointing forwards, sprawl out at the sides in a rather spider-like fashion; at the same time the front pair, which are not elongated, are widely opened somewhat as the jaws of a spider might be made to gape, and the hinder part of the body is bent up over the back, thus making some slight approximation to the shape of a spider's globose hind parts. Now as this is not the ordinary attitude of the caterpillar, but is only assumed when it is irritated, we may reasonably conclude that its assumption is intended as a means of defence, and it has therefore been supposed that this "terrifying attitude," as it is called, suggests to the creature's enemies a dangerous foe of the spider class and induces them not to molest it. It is true that the resemblance to a spider is far from perfect; in particular, the body is not so broad as a spider's would be, and there are only two pairs of legs extended instead of four; in fact, as Mr. Poulton, who has had good opportunities of watching this larva, admits, the "suggested monster" is "exactly like nothing upon earth, but is, nevertheless, most effective in its appeal to the imagination." At any rate, to an enemy gifted with some degree of imaginativeness, and approaching it in front, it would look sufficiently uncanny to suggest the idea of possible powers of harm-doing, and thus rouse feelings of suspicion and cause a would-be captor to hesitate before attacking it. Of course, if the enemy is shrewd enough to see through the deception, the fate of the larva is sealed, for it is in no way able to inflict injury or to protect itself

further, nor does it seem to be in the least degree an unpalatable morsel.

But as a few actual experiments are worth more than any amount of theorizing, we are fortunate in having records of the observed behaviour of this insect in situations of danger. Mr. Poulton placed one of them on a table and caused it to assume the terrifying attitude; he then drew the attention of a marmoset to it, but though this little monkey is very fond of caterpillars, and would seize with avidity those of an ordinary appearance, it hesitated for some time before proceeding to the attack, and when at last it summoned up sufficient courage, advanced in a very cautious manner. A lizard also manifested hesitation in a similar case, although, being a less intelligent animal, and therefore presumably less imaginative, it was not so strongly affected as the monkey. In this case the attitude was sufficiently influential to cause hesitation on the part of the marmoset, though it did not succeed in saving the insect's life. But possibly the result might have been different in the wild and open state of nature, where there would have been many other opportunities of obtaining food, and the preliminary reluctance might have developed into complete avoidance, and thus have been the means of preserving the caterpillar's life; and though no such foes as monkeys of any kind would encounter it in a native condition in this country, it is not improbable that birds, which would be its chief vertebrate enemies, might be daunted in the same way as the marmoset.

It has also been suggested that the spider-like attitude may be intended to frighten away ichneumon flies as well as vertebrate foes, not that parasites belonging to this group *never* attack spiders, but that they seem, as a rule, inclined to avoid them, and in fact they are very seldom found caught in spiders' webs. And further, it has been pointed out that the lobster moth caterpillar has a certain peculiarity of marking which would tend to protect it from ichneumons, even if they were not deterred by its fantastic appearance. On the first and second segments behind those to which the legs are attached, there is on each side a small pouch-like hollow of an intensely black colour, but capable of concealment by means of a triangular flap of skin. When the caterpillar is at rest, the black spots remain covered, but when irritated and alarmed the flap is drawn back and the black marks made to become conspicuous. Now, as ichneumon flies are apt to make small wounds on caterpillars, not only by their ovipositors, but also by their jaws and claws as they hold on trying to effect oviposition, and as the blood issuing from these wounds invariably forms a black clot, it is suggested that the appearance of these black spots on the lobster caterpillar may give the idea to an ichneumon contemplating attack that the insect has already been selected and has received the indwelling parasite, so that it is not available for the reception of another. It is much to be desired that experiments on this score could be tried with ichneumon flies, but for this we must be content to wait. Even with these beautiful adaptations, if they be such, we do not exhaust the means of defence possessed by this remarkable insect. Its brownish colour almost exactly resembles that of a dried and curled-up beech leaf, that being one of the trees on which it feeds, while in the time of its larval life, the late summer and autumn, such leaves would be likely to be about; and again, its long legs have been pointed out by Mr. Poulton, not only to be suggestive of spiders, but also, when folded up in the position of rest, to resemble very closely the little bunches of brown scales or stipules that hang down from the leaves of the beech tree. So this insect seems to be a special pet of Nature, the one frail child which is not fit to rough it, and to secure whose welfare, therefore, unusual precautions are required, its special need having led to the acquisition of quite special means of protection; and indeed, notwithstanding this, still it is not able to flourish and multiply to any great extent, for it is never other than a rare insect.

The six thoracic legs, as might be inferred from their form, are not the only, nor indeed the chief, means of progression possessed by caterpillars. Some of their hinder segments are furnished with pairs of fleshy and highly elastic pillar-like appendages, called claspers or prolegs, which are their chief, and sometimes their only means of support when they are on their food-plant. In all the larger species these organs have great grasping power, which is due to numbers of little hooks round their edge (Fig. 4), by aid of which the caterpillars can hold on so tightly that they will often endure damage rather than relax their grasp, the claspers themselves meanwhile remaining firm and tense by reason of the blood which is forced into them and kept there under pressure. The most common number of these equivalents of legs is five pairs, which gives the insect, together with the three pairs of thoracic legs, a total of sixteen points of contact with its base of support when at rest. One pair of prolegs is situated on the last segment; on the two segments in front of this, for a reason we shall see presently, there are none; and then the remaining four pairs occur on the next four segments; hence, remembering that there are in all twelve segments besides the head, it follows that between the last pair of true legs and the first pair of prolegs there are again two segments unprovided with appendages.

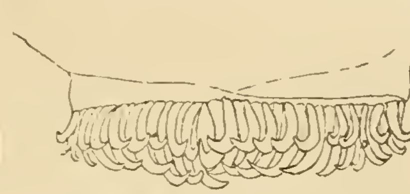


FIG. 4.—Microscopical preparation, showing hooks on proleg of Caterpillar of Lime Hawk Moth, magnified 16 diameters.

When a caterpillar thus equipped is resting stretched out at full length along a leaf or stem, it would generally hold on by all the sixteen legs; but some species rest clinging by the prolegs alone, the front part of the body being then elevated into the air. Special attitudes of rest are often characteristic of different groups. Thus, for instance, the caterpillars of several of the "hawk moths" elevate the front part of the body, and giving it a graceful curve, rest with it kept steadily, in a state of rigidity, in that one position, thus reminding one of the contour of an Egyptian sphinx. Amongst British insects this is best seen in the common privet hawk moth (*Sphinx ligustri*), which derives its generic name from the circumstance. After this caterpillar has reached its mid career, the sphinx-like attitude (Fig. 5) is often assumed as the ordinary position of rest, whence it would seem that some other reason should be sought for it than that of attempting to deter possible assailants, though conceivably the striking appearance of the caterpillar in this position might lead an animal which wished to make a meal of it to think twice before pouncing upon so stately

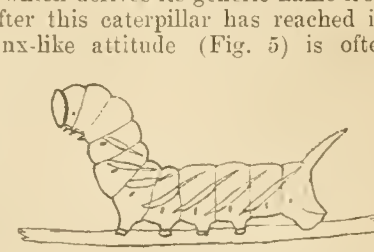


FIG. 5.—Caterpillar of Privet Hawk Moth, showing sphinx attitude, natural size (after Poulton).

and dignified a figure. Mr. Poulton indeed would reduce it to a mere question of mechanics. He points out that the attitude is only occasionally seen in the first two stages of the caterpillar's life, and that after that period it becomes much commoner, being most marked in the fourth stage, *i.e.*, the last but one. At this time the caterpillar usually rests upon the mid-rib of a leaf on its under surface, and later on, on upright stems, with its head towards the top of the stem. In either case, as soon as the front part of the body is released from contact with its support, it would naturally, by reason of gravitation, tend to fall backward, and the curved position would therefore result as a compromise between this tendency and the strain put upon its muscles in the attempt to neutralize the effect of gravity by becoming stiff and rigid. But though this explanation would no doubt account for much, it would not furnish any reason for the lifting of the body in the first instance, nor explain why the insect is not satisfied with doing as so many other caterpillars do, and indeed as it did itself when younger, namely, resting stretched out at full length on its support, and holding on by all sixteen legs. Amongst British insects we find some sixteen species of hawk moths, but they do not all assume the sphinx-like attitude, which is not marked in those that live in trees, while most of those that feed on low plants do not exhibit it.

When a sixteen-legged caterpillar, resting comfortably at full length, wants to begin to walk, it first advances the last pair of claspers, raising them and setting them down again nearer the last pair but one. This can easily be done, for it will be remembered that the two preceding segments have no legs, so that there is always a larger space between the last two pairs than between the others. Then, while it holds tight with this last pair, the others are successively raised from behind forwards, and planted firmly in the same order a step in advance, the legs of the opposite sides being, of course, moved together, and not alternately, as the perfect insect would do; at the same time a sort of wave of muscular movement travels along the body. The wave at last reaches the thoracic segments and the front part is thus moved on. The operation is repeated again and again with considerable rapidity, and thus the caterpillar manages to get over a good deal of ground in a short time, the actual pace depending, of course, to some extent upon the species.

Some caterpillars, such as those of the rare but beautiful Kentish glory (*Endromis versicolor*), and the moths called "prominents" (*Notodontidæ*), do not make much use of the last pair of claspers, and habitually rest with them lifted up and not adhering to any support. In other cases, they are not only unused but actually absent, and in their place we find a pair of stiff appendages forming a kind of tail, which is of no use for prehensile purposes. This is notably the case in the larva of the puss moth (*Dicranura vinula*). This extraordinary insect, which is scarcely less remarkable in form than the "lobster" itself, is a common species and may be found in the summer feeding on the leaves of willow and poplar trees. It throws the front part of its body into a somewhat sphinx-like attitude, and at the same time elevates the last three segments, which taper towards the tail and terminate in a pair of stiff tubular sheaths, prickly on the outside. From within each of these there can be shot forth rapidly a long, flexible, whip-like organ (Fig. 6) of a beautiful pink colour, cylindrical and hollow, but closed at the extremity. It is moved out and in much in the same way as the tentacles of a snail, that is, by a process of turning inside out and the reverse. The mechanism by which this is effected is twofold; it is extended by having blood forced into it, and as long as it remains full it retains its form and length, being at the same time capable of being flourished about like the lash of a whip; it is drawn back again by means of muscles which run up its tubular cavity, and are attached at its apex. The tip is first drawn in, and then, following it, more and more of the column, till the whole is withdrawn into the sheath. The action can be easily understood if one imagines the finger of a glove to be fully extended, and a thread to be passed up it inside and fastened to the top; on pulling gently at the thread, the tip descends into the hollow of the finger, and thus the whole finger, if the sides remain sufficiently firm, is gradually drawn back in a reversed position into itself and towards the palm, that surface which was at first an outward convexity now becoming an inward concavity. Of course, if means of exit were not provided for the blood contained in the whip-like filament, the muscular contraction would fail to withdraw it, as may be shown by tying a silken thread round its base when it is erected, so that the blood cannot return into the body, and then it will remain extended as long as the ligature is present.

Fig. 6.—Whip and sheath from tail of Caterpillar of Puss Moth, magnified 4 diameters (after Poulton).

Mr. Poulton has made very careful microscopical sections of this little organ, and has shown that it is most exquisitely constructed. The pink colour is due to a layer of thickish cells of spindle-shaped outline placed beneath the colourless outer layers of the skin. A little beneath this again lie bands of muscular fibres in the form of a hollow cylinder, and within this is a band of nerve fibres and a number of nerve cells, all the interspaces being filled with blood. The large comparative size of the nervous portion indicates the great sensitiveness of the organ, and accounts for the wonderful rapidity with which it can be moved. The whole organ in fact is extremely delicate, and slight damage done to it, especially at the point where it joins the tubular sheath, would entirely destroy the power of shooting it forth; this apparently is a misfortune that usually happens sooner or later, for the adult insect generally loses control over its filaments, and they may then be found lying helplessly within the sheaths. When, however, they are in good working order, the insect, as soon as it is touched, dashes its head round in the direction of the irritation and immediately shoots forth the tail filaments and flourishes them about; so that there can be little doubt that their purpose is to scare away ichneumon flies that may chance to alight upon its back. How great a need there is for this is evidenced by the fact that, notwithstanding this means of defence, a considerable proportion of the caterpillars perish through the attacks of a large reddish-yellow ichneumon; without this and other defences which they have, no doubt the mortality would be much greater. Like the puss moth, the "lobster" caterpillar also possesses no terminal claspers, but a pair of long rod-like appendages instead; but they are not hollow, and therefore possess no filaments. They help to heighten the effect of the "suggested monster" when the tail is turned forward and brought near the head, by becoming separated and thus simulating a pair of antennæ; but when the caterpillar is lying extended they are brought close together, and then simulate the stalk of the withered beech leaf which the caterpillar suggests. Other caterpillars belonging to the same group, those of the "hook-tips," so called from the hooked fore-wing of

WEST.

4th April, 1892.—Exposure, 1h.

6th April, 1892.—Exposure, 1h. 5m.

7th April, 1892.—Exposure, 50m.

S.

PHOTOGRAPHS OF SWIFT'S COMET. Taken by Professor E. E. BARNARD at the Lick Observatory with a 6-inch camera of 31 inches focus.

Enlarged $2\frac{1}{4}$ diameters. Scale of this plate 1 inch = 1·2°. The Stars are represented by short lines parallel to the direction of the Comet's motion, as the camera was made to follow the motion of the Comet and not the diurnal motion of the Stars.

the moths, have the body terminating in a single point, while still others, the "prominents," have the usual pair of terminal claspers, but make no use of them, seeming as though they were gradually losing the power of doing so.

(*To be continued.*)

PHOTOGRAPHS OF SWIFT'S COMET.

By Prof. E. E. Barnard, of the Lick Observatory.

THESE three photographs, selected from a series and enlarged $2\frac{1}{4}$ times, clearly show the remarkable changes the comet was subject to when near perihelion. A brief description of the pictures, pending a more detailed discussion, may not be out of place here.

These were made with the 6 in. Willard lens, strapped on to the tube of the $6\frac{1}{2}$ in. equatorial, which was used as a following telescope. The focus of the Willard lens is 31 inches. As the comet was moving rapidly among the stars, the clock-work, which corrects for the ordinary diurnal motion, would not serve to follow the comet. The nucleus was therefore brought to the intersection of cross wires, a high power eye-piece being used on the $6\frac{1}{2}$ in., and the telescope was constantly shifted by hand with the slow motion rods, so that the nucleus was always kept perfectly bisected, the clock simply correcting for the diurnal motion. The light of the comet seems to have been very strongly actinic, as will be seen from the amount of detail shown with comparatively short exposures, very little of which could be seen with the eye and telescope. Cloudy weather interfered greatly throughout the time of the comet's greatest brightness. After a long cloudy spell, I had examined the comet on April 3rd, when its tail was traceable for 20° with the naked eye. The 12 in. achromatic showed the tail, near the head, to consist of two thin streams of slightly divergent light, with scarcely any nebulosity between. There was certainly no third stream.

The Photographs.

April 4th, 15h. 30m.—16h. 30m. It will be seen in this picture that a third branch had made its appearance since the 3rd, in between the two previously seen. This central branch is shown in the photograph to be crested with a fine bright line, more or less curved and broken. For a short distance from the head a second line lies close by and parallel to it. To the north of the northern branch, there are one or two hair-like, dark spaces, which appear darker than the sky near; they are doubtless simple rifts in faint diffused nebulosity surrounding the comet. It will be seen that the tails of the comet readily break up into quite a number of separate branches at some distance from the head. The star trails, representing the direction and amount of motion of the comet during the exposure, are pear-shaped in this picture. This is due to the fact that the exposure was begun when the comet was near the horizon and the light of the stars was more or less absorbed by the dense atmosphere. This peculiarity serves to mark the beginning of the exposure and the direction of motion of the comet, the motion being in the direction of the large end of the trail.

April 6th, 15h. 35m.—16h. 40m. This picture shows the remarkable changes undergone by the comet in the short interval of two days. There is very little or no resemblance in the comet for the two dates. It will be seen that the separation in the tail makes a rather quick bend near the head. There are unequal bright areas in the main tail, some of which are suggestive of the remarkable changes seen in the picture for April 7th. There is a little incident connected with the history of this photograph of the 6th of April that may be of popular interest. On this date it was necessary for me to go to San José to fulfil an engagement to lecture that night (being one of a series of six lectures for the University extension). After the lecture, at 10h. 30m. p.m. I hired a horse and buggy and drove up the mountain, 27 miles, arriving at the observatory at 2.30 a.m., and between that time and dawn secured this photograph. On this date, to the naked eye, the tail was about 25° long, the head being equal to a star of the third magnitude.

April 7th, 15h. 45m.—16h. 35m. This photograph was made partly in moonlight and partly in dawn. I need scarcely call attention to the unique appearance of the tail on this occasion. There is still less resemblance to the former appearance of the comet in the short interval of twenty-four hours. A number of narrow branches are now shown in the tail near the head, the middle or main one of which has a curve in it some distance from the head. The most remarkable and unique phenomenon, however, is the apparent development of a secondary comet 2° behind the head in the main branch. There is at this point a great enlargement or swelling, which is gradually brighter in the middle, and from which a new system of tails seems to branch out. A photograph made the next morning in full moonlight showed only a portion of the tail close to the head, the sky being too bright for the photography of faint objects. The scale of these enlargements is 1 inch = 1·2°.

It seems to me that these photographs are a revelation to us. We are familiar with the rapid changes that comets sometimes undergo, but if these three pictures, so close together in point of time, had been drawn by the most competent observer, most astronomers would probably have attributed their remarkable differences to the unskilful hand of the artist, for there is absolutely no resemblance among them. The accuracy of the photographic plate, however, is unquestionable, and these pictures therefore give us an insight into the rapidity and vastness of cometary changes little dreamed of before.

In examining this series of photographs the idea has been very forcibly impressed upon me that there was possibly, in the case of this comet, a rotation of the tail upon an axis through the nucleus, in a comparatively short period. It is to be regretted that cloudy weather broke the series to such an extent that it is not possible to settle this question; still it is a point that must be closely looked after in our next large comet.

Mount Hamilton, August 31st, 1892.

ON THE FORMS OF COMETS' TAILS.

By A. C. Ranyard.

THE beautiful photographs, for which we have to thank Prof. Barnard, show some remarkable irregularities in the streams of matter driven away from the nucleus and from the sun. There is not merely a variation in the brightness or density of different parts of the tail, such as might be caused by the matter being driven away from the nucleus in varying quantities at successive instants, but the edges of some of the streamers appear to be distinctly curved or bent in an irregular manner. This is particularly noticeable in the southern edge of the great streamer, photographed on the 6th of April.

We do not know how far the photographic record obtained may be affected by the motion of the matter of the tail during the 65 minutes for which the photographic

plate was exposed, but any motion of a bright mass of matter in the tail in a direction away from the comet's head during the exposure would tend to obliterate such irregularities, and give a straighter appearance to the streamers or jets.

There can be no doubt about the existence of the irregularities referred to. The southern (or right hand) edge of the great streamer, photographed on the 6th of April, is very distinctly notched or bent inwards at a distance of about 4° (that is, at a distance of about $3\frac{1}{2}$ inches on the scale of our plate) from the nucleus. A little above the notch the narrow stream of luminous matter which forms the southern edge of the great streamer divides into two, or forks. This is clearly shown in our plate at a distance of about 5 inches from the nucleus, and it is still more clearly shown on the glass enlargement from the original negative which Prof. Barnard has kindly sent to me. There is also just traceable on the plate, and distinctly visible on the glass enlargement, a narrow stream on the inner or northern side of this branching structure, and several faint structures which appear to branch or fork somewhat after the manner of the structure on the edge of the great streamer.

There is also a distinct bend in the edge of the great streamer of the 6th of April nearer the nucleus at the place where it divides from the fainter group of rays which form the southern half of the comet's tail. The division between the great streamer which forms the northern half of the tail and the group of rays which forms the southern half is continued as a very narrow black line towards the nucleus, and this line makes a very obtuse angle with the rest of the southern edge of the great streamer. On the northern side of this narrow dark line is a bright forked structure, the branches of which trend away from the nucleus, similarly to the branches of the forked structure referred to above.

The branching structure last described has, as shown on Prof. Barnard's glass enlargement, a form which reminds one of a solar prominence, and it gives a clue which may possibly explain the irregularities in the edges of the streamers. If during the rapid evolution of vapours in the neighbourhood of the nucleus, gas is evolved in irregular or intermittent quantities, and is projected outwards, we should expect an uprushing mass of gas in passing through a resisting medium or atmosphere about the cometary nucleus to take the tree-like prominence forms which uprushing masses of gas take upon the sun, and such prominence forms, as they are driven away from the sun and nucleus, would give rise to irregularities in the edges of the streamers as well as to the mottled appearance of the tail which we see in Prof. Barnard's photographs.

In a waterfall the foam on the surface tends to arrange itself into parabola-like curves with the apex downwards, because the stream is retarded at its edges, and a wave on the surface or any other line across the stream moves more rapidly at the centre than at the edges of the stream, but there are no such transverse markings in the mottling of the tail of Swift's Comet. The mottling seems to be disposed in irregular masses, with here and there an appearance of branching in a direction away from the nucleus. Such irregularities are worthy of the closest study, because deviations from general laws have always formed stepping-stones to further discovery.

Our first step is to make sure of facts. There can be little doubt that there is a decided bend in the lower part of the main stream photographed on the 7th of April, about half-way between the nucleus and the curious knot or branch in the main streamer, which Prof. Barnard describes as a second comet; possibly it may have been a secondary or attendant comet, seen through the tail of the large one, for such attendant comets have been observed before, though, as far as I am aware, they have not appeared in the midst of the tail of a large comet and, apparently, intimately associated with one of its streamers. The fact which chiefly weighs with me in concluding that the structure was probably a second comet is that the bright point which seems to form the nucleus of the little comet is not stretched out into a line by the motion of the camera in following the large comet—as all the stars are. The nucleus of the little comet was therefore a bright point which was moving with the large comet. But the rest of the small cometary structure has very much the appearance of being an irregular branch from the main streamer of the large comet.

It is much to be regretted that we have not other photographs taken an hour or two before and an hour or two after Prof. Barnard's photograph on the 7th of April, and it would also have greatly added to the interest of his work if Prof. Barnard had taken photographs of the comet with short exposures of a few minutes before and after the long exposures. We should then, no doubt, have been able to trace more structure in the head or nucleus of the comet, which is now obliterated by the long exposure. But Prof. Barnard did not expect to find the light of the comet so actinic. Curious irregularities have been noticed in the tails of other comets, notably in the tail of the great comet of 1882. There is a curious increase of brightness and striation towards the end of its tail, which was drawn by more than one observer and was photographed on October 20th at the Cape Observatory. The original negatives of this comet have been deposited by Dr. Gill in the library of the Astronomical Society at Burlington House, and are well worthy of examination.

There is evidence which can hardly be doubted tending to show that the tail of Donati's great comet, which appeared in 1858, did not lie accurately in the plane of its orbit. According to Prof. G. P. Bond, who collated the drawings and observations of a great number of European and American observers of this comet, and discussed them in a very valuable monograph which was published as Vol. III. of the "Harvard Annals," the axis of the tail of the comet was inclined at an angle of nearly 4° to the plane of its orbit; and during nearly the whole of its apparition there was a contrast in the density of the two branches of the tail, which remained unchanged when the earth passed through the plane of the comet's orbit on the 8th of September—a fact which points to the conclusion that there was no revolution about an axis, of the nucleus, or swarm of stones, from which the streams of matter forming the tail issued.

The curvature of the tail or tails (for Donati's comet had three), as well as the gradual decrease of density of the tail with increase of distance from the nucleus, points to the conclusion that matter is continually streaming away from the nucleus, and is driven away from the sun. Matter driven backwards into space with very great velocity, compared with the velocity of the nucleus in its orbit, would evidently form a nearly straight tail in the prolongation of the radius vector (or line joining the nucleus with the sun), while matter driven backwards with a velocity comparable with the velocity of the nucleus would drop behind the radius vector, and form a tail curving backwards in the plane of the orbit, the curvature being more and more apparent the slower the velocity with which the matter of the tail was driven away. Thus the different curvatures of the tails of comets exhibiting more than one tail is accounted for.

Prof. Bredichin, whose theory has been much quoted, is inclined to call in an unknown electrical repulsion,

differing for different materials, to account for the different velocities of repulsion, and the different curvatures of cometary tails.

The particles of which comets' tails are composed are evidently very small, for they are small in average diameter compared with the wave-length of light—this is rendered evident by the fact that the light dispersed of comets' tails (except in the neighbourhood of the nucleus, where a gaseous spectrum is recognizable) is generally strongly polarized—and it seems to me that we may satisfactorily account for the repulsion of such small particles from the sun without calling to our aid any unknown force, but by merely considering the repulsion which would be caused by evaporation taking place from the side of the particles exposed towards the sun. In a paper published in KNOWLEDGE for February 16th, 1883, I have discussed the accumulated effect of the minute recoils which must accompany evaporation as one molecule after another is thrown off from the surface of a small heated body towards the sun. When three-fourths of a heated particle have been thus evaporated towards the sun, the velocity of the remaining one-fourth away from the sun would be greater than the molecular velocity of evaporation, but the irregularities in the tails of comets referred to above show that we have other forces to consider, which slightly vary the form that cometary tails would assume if the matter of the tail were only acted upon by repulsive forces, in directions away from the sun and away from the nucleus.

Science Notes.

Part XIII. of "The Old and New Astronomy," containing the last portion of the chapter on the Stellar Universe, with the title page, preface, and index, is now in the hands of the public.

Fresh applications of photography are continually being reported. The camera has revealed much with regard to animal locomotion; the nature of the horse's movements in trotting and galloping have been revealed, much to the surprise of many animal artists. So with regard to the movements of birds' wings, which have been photographically registered. But photography has recently been applied to record the movements of the growing parts of plants, and some curious results have been obtained. In the case of the hop-convolvulus, the movement of the young stems consists of a succession of irregular, circular, or elliptical curves, which vary every moment, even in their direction. These movements are caused by unequal growth in different parts of the stem. Even when asleep, plants move, not, as was formerly supposed, interruptedly, but continuously.

The apple-rot is produced by a parasitic fungus, which appears to be the same as that which causes the ripe-rot in grapes. The disease is infectious, as anyone who keeps a number of apples may observe; for it is passed on from one apple to another. But if those who gather the apples, and pack them for market, were a little more careful in handling them, we should not have so many affected. Any slight bruise on the skin is sufficient to cause those disagreeable brown spots which cannot be eaten.

In a recent number of *Nature*, Mr. H. H. Dixon describes the walking of certain insects (*Arthropoda*) as revealed by photography. The paper is most interesting.

Congress has this year refused, for the present, to sanction any further expenditure on the United States Geological Survey. Field work has been suspended, but it is to be hoped that the survey will not come to an end, for such an event would be little short of a catastrophe. English geologists have learned a great deal from the splendid monographs and bulletins that have been so generously supplied to our libraries and museums, and all must admire the ability and thoroughness with which most of the work has been done. America is such a huge country, and contains such magnificent illustrations of physical features produced on a great scale by denudation, volcanic action, earth-movements, &c., that it would be a great pity if no further geological surveying were undertaken. It is well known, however, that certain abuses have been going on, and, if these can be removed, probably this important work will some day be renewed.

An interesting discovery is reported in the November number of the *Mediterranean Naturalist* by Mr. J. H. Cooke, namely, the finding of a portion of an Arctic bear's jaw (*Ursus arctos*) in pleistocene strata, Malta. It had previously been concluded, from the fact that elephants' bones discovered there were much gnawed, that there must have been carnivorous animals living there at that period, but no remains of such could be found. Now Mr. Cooke has carried out some excavations in the Har Dalam cavern, and found the evidence that was wanted, viz., an entire *ramus* of the lower jaw of a bear with its canine molars. Besides the bones of elephants, those of the hippopotamus, stag and a large dog were found.

A new genus and species of blind cave salamander is reported by Mr. L. Stejnger in the "Proceedings of the United States National Museum." The discovery of such an animal in North America is regarded by the writer as a most important and interesting event.

Science contains an interesting paper on the food of humming-birds by Dr. Morris Gibbs. Although he has dissected many specimens, he found no evidence that they lived on insects. Possibly, when flowers are scarce some species of insects may be taken, but when flowers are abundant the ruby-throat of Michigan lives on honey.

The important work by Mr. Saville Kent, on "The Great Barrier Reef of Australia," announced by Messrs. W. H. Allen & Co., is a book to which naturalists are looking forward with considerable interest. Geologists will also be interested in it, both on account of the problems connected with Darwin's theory of coral-reefs (now abandoned by Mr. Murray and others), and because there are fossil reefs—probably of the same nature—buried in several well-known English formations, viz., the silurian, devonian and carboniferous, and the jurassic series. But for some reason or other the biological aspects of coral-reefs have been somewhat neglected, although their pools and lagoons are full of varied forms of life. The author, who is fishery inspector of several of the Australian Colonies, is a recognized authority on lower forms of life, such as infusoria, sponges, and corals. The book will be illustrated by 16 coloured quarto plates and 48 photographs. The former have been copied from paintings from nature by the author. The photographs show large areas exposed at low tide, and some submerged parts of reefs.

A very wonderful new "extinct monster" has recently been set up in the Natural History Museum, Cromwell Road. It is to be seen in a handsome glass case at the end of the fossil reptile gallery, among the ichthyosauri and other extinct types. This fine specimen, the only complete skeleton known, was discovered last year by Professor H. G. Seeley, in South Africa, and has been

described by him in a paper read before the Royal Society in 1891. In length it is nearly eight feet. The bones are all massive and strong, its legs are short, and when walking the creature probably had a somewhat sprawling gait, like a crocodile or lizard. The head is much like that of a salamander, or even a frog. It is difficult to say what the animal was most like, but it probably was amphibious. Altogether it is a strange and now wholly extinct type, with a resemblance to a salamander, and yet in some ways aping a modern mammal. The name *Pariasaurus Baini* has been given to this remarkable reptile, and it was found in the Karoo formation (probably of triassic age), near Tamboer Fontein.

More than twenty years ago Mr. Alfred Russell Wallace told the Zoological Society of a so-called "horned toad," which was said to squirt blood from its eyes. The information was derived from a correspondent in California, and probably, at the time, many who heard the news were disinclined to believe in it. But lately the matter has been confirmed beyond all doubt. Mr. O. P. Hay records, in the "Proceedings of the United States Museum," that two boys from Texas showed him some lizards belonging to the genus *Phrynosoma* (popularly known as "horned toads," and distinguished by a remarkable frill round the neck). These, the boys declared, would sometimes, when teased, squirt blood out of their eyes. At the time he was incredulous, but lately he has proved that the boys were right. A living specimen of this lizard, sent from California, was in the museum near his desk. About August 1st it was shedding its outer skin, and as the process seemed a somewhat difficult one on account of the dryness of the skin, he proceeded to give the animal a wetting. For this purpose he took it up and tossed it into a basin of water. To his great surprise, there suddenly appeared on one side of the basin a number of spots of red fluid resembling blood. By means of a microscope this fluid was proved to be blood, and on another occasion Mr. Hay was able to observe that the blood came directly out of the right eye. Truly, there is no end to the possibilities of Nature.

The late Poet Laureate was one of the few poets who introduced into his works the results of modern science, and clothed them in poetic form. He thus not only showed his sympathy with the age, but set a good example to others by indicating that the love of Nature should go with a desire to understand her workings, and the laws by which all things are governed. Science, rightly considered, is full of romance, and many a scientific truth might, if expressed poetically, be found quite as fascinating as some old-fashioned romances and fairy tales. The familiar saying, "Truth is stranger than fiction," is often on our lips, but few seem to act upon this truth by endeavouring to bring out the poetic aspects of science. Geology teaches us that the former history of our earth is, as it were, a great drama. To the geologist the world is a stage on which the various scenes in this great earth-drama have been enacted. The players were the pre-Adamite animals whose remains we discover embedded in the rocks beneath our feet; while old-world forms of vegetation gave beauty and completeness to the scenery. Then there is the romance of Astronomy, which appeals very forcibly to some people. But every science has its romance, from Chemistry, with its ever-dancing molecules, to Biology with its wonder-working protoplasm—the basis of all life.

Lovers of Tennyson will easily recall certain passages in his writings in which scientific discoveries (and sometimes theories) are introduced like precious stones set in the gold of verse. The following examples occur to us, but doubtless our lovers of Tennyson will recall many others. In "Locksley Hall," a lovely description of the Pleiades is followed by two lines referring to the untold æons revealed by Geology :—

"Here about the beach I wandered, nursing a youth sublime
With the fairy tales of science, and the long results of time."

Geological truths are again referred to in "In Memoriam." The following lines refer to former geographical revolutions :—

"There rolls the deep where grew the tree.
O earth, what changes hast thou seen!
There, where the long street roars, hath been
The stillness of the central sea."

There is no doubt at all that land and water have in many parts of the earth over and over again changed places, and large tracts once under the sea have been raised up into dry land; as, for example, the chalk round London. This formation must have been slowly formed in a fairly deep sea, say of 2000 to 2500 fathoms. But the latest researches indicate that probably the very deepest parts of oceans have never been dry lands, so that the expression "the central sea" is not quite in harmony with modern teaching. But with this slight qualification the geology here introduced is quite correct.

No exception, however, can be taken to the next verse, in which the great work of "denudation" is beautifully expressed. Geologists know that great mountain ranges have, in the course of ages, been worn down by "rain and rivers" until nothing but a mere wreck of their former grandeur is left. Tennyson thus expressed this important truth :—

"The hills are shadows, and they flow
From form to form, and nothing stands;
They melt like mists, the solid lands,
Like clouds they shape themselves and go."

In "The Princess" are to be found several allusions to Geology, Astronomy, Electricity, &c.

RECENT TRADE AND THE NATION'S DRINKING HABITS.

By Alex. B. MacDowall, M.A.

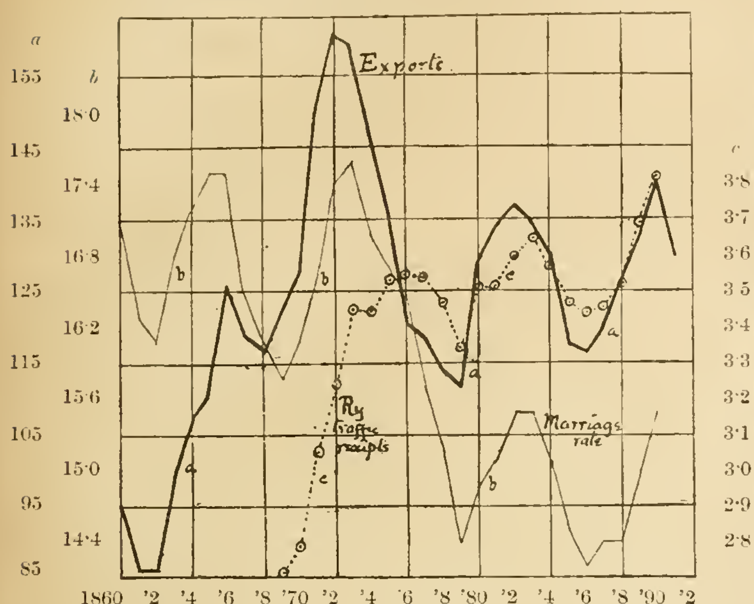
We have long been used to hear a great deal about depression of trade and bad times. Some seem to think depression is the chronic state of trade, yet while it may be said, in general, that trade has been depressed for many years past, one may note certain well-marked fluctuations in the last twenty years; trade becoming more brisk for a few years, then less brisk again. Let us try to form a clear idea of those waves by means of curves.

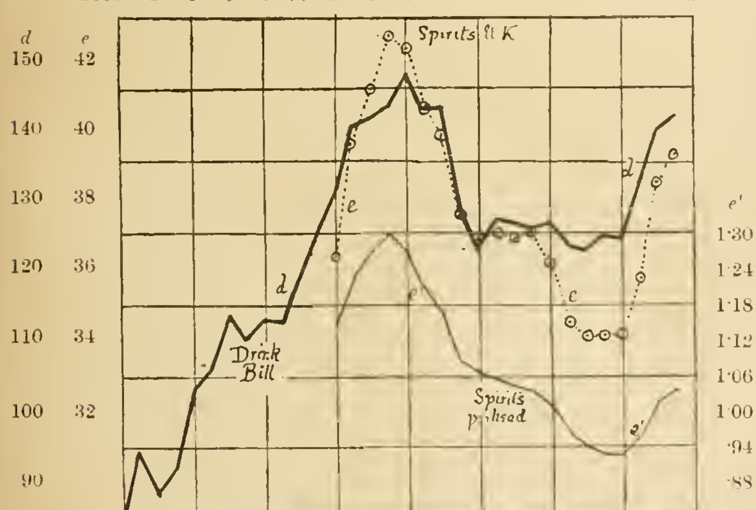
The method of curves is now well known, and we need not stop to explain it. While the curves here given are all comparable together, the comparison must not be pushed too far; for while they have all the same time scale (the horizontal one), they have each a different vertical scale (indicated by letters *a*, *b*, *c*, &c.). Thus one may superpose (so to speak) several diagrams, economizing space. In these curves attention is especially asked to the waves, and the times of their high and low points.

How shall we measure trade? Our exports afford a very fair measure. We take, then, the values of exports of British and Irish produce per head of population (and in shillings) since 1860, and thus obtain the curve marked *a*. This rises rapidly, with a small crest in 1866, to a very high crest in 1872; then goes down to 1879; up again to 1882; down to 1886; up to 1890; and last year it again goes down. We are now on the descending slope. The time of that high maximum will be remembered by many, when our commerce rose (as Mr. Gladstone put it) by leaps

and bounds. It was about 1874 that the depression set in from which we have suffered more or less since.

A good reflection of the state of trade is offered by the marriage rate. Naturally, when money is plentiful, more people marry; when scarce, fewer. Here (*b*) is the marriage rate curve for England and Wales. Its waves correspond very well with those in the curve of exports. There was a great fall in the rate from that very





prosperous time, and the rate has not since recovered its previous position.

Look at the matter in another light – (curve *c*), that of the railway traffic receipts per mile of railway (the unit here is a thousand pounds). The story is similar. Notice, however, that we have the first wave culminating in 1876, four years later than the great wave of exports.

The exports curve does not enlighten us much as to general increase or decrease of our trade, for this reason, that it is a curve of values, and there has been a great fall in prices since the seventies. Curves of volume would be more instructive in this respect.

It is interesting to notice the intervals between the wave-crests. They are, in this period, about eight or nine years; taking the dates of the marriage rate curve, we have 1865 to 1873 (eight years), 1882 (nine years), then, say, 1890 (eight years). Should this recurrence continue, we should expect a crest about 1898 or 1899, and the bottom of the trough, into which we are now descending, a year or two from the present. The causes of trade depression have, of course, been much discussed; over-production, protection abroad, bad harvests, disturbances of the currency, are some of the causes that have been suggested. But we will not here enter into this question.

It is well known that our national consumption of alcoholic liquors depends very much on whether trade is good or bad. Good times of trade are bad times of drinking, as a rule. Here is a curve (*d*) which shows our drink bill since 1860 (in million pounds). It rises rapidly to 1876 (when railway traffic receipts were at a wave-crest), after which comes a fall. The next trade wave is but slightly represented, but since 1886 there has been a pronounced rise corresponding to the trade wave which culminated in 1890.

A curve (*e*) of spirits (British and foreign) retained for home consumption in the United Kingdom shows very much the same thing. (This refers to potable spirits alone from 1877, and to millions of gallons.) Another curve (*e'*) shows the spirits per head of population (in gallons). Both of these culminate in 1875.

Now it is often urged, somewhat confidently, that an increase of consumption of spirits, or of the drink bill, is no proof of an increase of *excessive* drinking; people are merely consuming more in a moderate way, it is said, in accordance with their easier means. It is a significant fact, however, which we meet on turning to the subject of apprehensions for drunkenness (of which we give a diagram), that we find waves in these closely corresponding to the waves of trade. Here, for example (curve *f*) is a curve showing (in thousands) the total apprehensions for this offence in England and Wales since 1871. It has a wave-crest in 1876, then in 1884 (a little after the trade wave-crest), and ends at, or near, another crest. Again, look at the three curves *g*, *h*, *i*, which relate to *convictions* for drunkenness in three great police divisions of England and Wales; we again find correspondence. The curve *j* (once more) shows apprehensions in London. We may note that it continues rising some time after the culmination of the very prosperous time to 1878, and that the next wave is hardly represented in it.

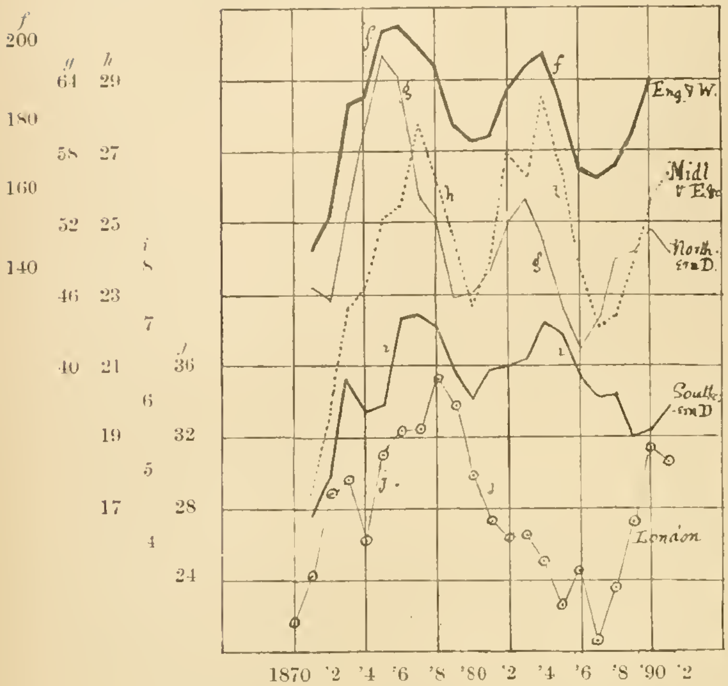
The general question as to whether drunkenness is increasing or otherwise is a difficult one. Taking only short periods, one may easily make mistakes. Thus, in 1880, a person might say, "What a vast improvement lately!" and in 1890 or 1891 it might be said, "How terribly we are deteriorating!" These would be but half truths.

The curves of drunkenness here given appear plainly to mean some improvement in the period considered. For they all either keep about a level, or tend to go down, and we have to remember that the population has increased; so that unless we are to make the unlikely supposition of a general increasing laxity in apprehension, there are fewer people, proportionately, who transgress and attract the attention of the police.

On the other hand, if we measure drunkenness by the consumption of spirits in a long period of years (say fifty), we find the curve swaying now to the one side, now to the other, yet, on the whole, departing in no continual way from the curve of population (or from the proportion of one gallon per head).

If some improvement is perceptible, it cannot be said that the great tide of intemperance is being effectively stayed. Should another time of good trade come like that before 1872, dare we expect that our drink curves would

not mount as much proportionally as they did at that time?



Convictions and Apprehensions for Drunkenness.

The great rise of public sentiment in favour of temperance is a remarkable feature of our time; and if with so many of our best citizens strenuously at work contending with this national vice, the evil still continues so bad, we have thankfully to remember how much worse it would have been if these efforts had been withheld.

These diagrams (in the present writer's opinion) afford an easy means of testing statements of various kinds which are made on the great drink question.

Notices of Books.

Théorie du Soleil. Par A. Brester, Jz. Amsterdam, 1892.—There can be no doubt that the physical constitution of the sun still remains to a large extent enigmatical. Experience is, indeed, baffled by the intensity and scale of action of the forces concerned, and the extension of inferences beyond the sphere where they are certainly legitimate to far outlying regions is always a perilous process. The subject hence bristles with difficulties which, in many cases, seem to become more formidable the more closely they are examined. Efforts to remove them, individually and collectively, are continually renewed, not altogether fruitlessly, though with only partial success. The work before us aims at providing a complete solution of the entire problem. It is evidently the outcome of long thinking based on painstaking research, and as such deserves respectful consideration. Yet we doubt whether Dr. Brester has really found the *mot de l'enigme.* His theory is essentially a chemical one. Stars and suns, as he says, "being after all nothing but enormous bubbles of incandescent gas, and incandescent gases being precisely the substances with which modern chemistry is best acquainted, it seemed to me that in studying such bodies chemical science should, in the first place, be consulted. If these vast bubbles are composed of matter in the last stage of disaggregation—of matter, that is to say, evaporated, dissociated, and dilated by transcendent heat—it belongs to chemistry to predict the course of change in this attenuated matter as it radiates away its heat."

Starting from this postulate, Dr. Brester contrives to "save" (as the old Greeks used to phrase it) all the solar phenomena. His sun, he truly remarks, is like the suns of most other theorists, only it behaves in a totally different manner. Assuming the absence of external causes of agitation, he concludes the workings of the great machine to be carried on in almost absolute physical tranquillity. Neither cyclones nor eruptions (in the material sense) occur in his sun; uprushes and downrushes are alike absent; the stratification, in the order of their densities, of the vapours forming the solar photosphere, subsists perennially undisturbed. To account for the apparently violent commotions observed in the solar atmosphere and its surroundings, the dissociating power of heat and the effects of chemical affinity are alone invoked. The progress of refrigeration, it is argued, incessantly brings about partial combinations of the most elementary principles of matter, and this being accompanied by the development of heat, dissociation again ensues, and so the play of forces is kept up. Their play, moreover, is rendered more or less spasmodic by the prevalence of what our author terms "super-dissociation." This state is reached by atoms cooled down below the point at which their strong inherent affinities would bring about their combination were it not for the copious presence of atoms of different natures, and when they are eventually enabled to come together it is in such numbers, and with so great violence, as to cause a "heat eruption." Such heat eruptions, by dissolving the photospheric clouds, occasion spots, while the coronal rays, as well as "white prominences," mark regions where a lowering of temperature has permitted the formation of a glowing mist capable of reflecting sunlight. Thus, no transport worth mentioning of ponderable matter is involved even in the most seemingly tumultuous of the solar crises. Calm really prevails in strata through which hurricanes appear to sweep. Prominences, accordingly, are maintained to be mere evanescent illuminations. They are the products of rapidly propagated chemical action. But if this were the case they would give a continuous spectrum. Hydrogen ignited by combustion shows no trace of its characteristic rays. Moreover, the rate of propagation of chemical action through a gaseous mass is vastly too slow to account for the line displacements in prominence-spectra, usually admitted to be due to motion in the line of sight.

Dr. Brester's elaborate discussion, none the less, raises several valid objections to current theories, and insists upon incongruities too apt to be lightly passed over. It serves, at the least, a useful ventilating purpose. Copious annotations, too, in the shape of foot-notes afford in themselves a very fair conspectus of the literature of the subject, and add greatly to the value of the book. It is, indeed, only the first instalment of a larger work, the second and third parts of which will apply the principles now unfolded to elucidate the natures of stars, nebulæ, and comets. They were employed to explain stellar variability in an essay published in 1889, but that intricate theme cannot here be entered upon. Agnes M. Clerke.

Vegetable Wasps and Plant Worms. By Dr. M. C. Cooke (Christian Knowledge Society).—Under this somewhat romantic heading Dr. Cooke, the well-known authority on cryptogamic plant life, gives what is described in the subsidiary title as "a popular history of entomogenous fungi, or fungi parasitic upon insects." The book is essentially a compilation which has been made in the interests of two classes of readers, mycologists and entomologists, to both

of whom it should be welcome as summarizing present knowledge and indicating the advantage, and indeed necessity, of co-operation between students in different branches of science. Much loss has hitherto accrued to science in consequence of the lack of such co-operation. The entomologist, it is true, describes and identifies his insect accurately enough, but when he comes to the fungus which has spoilt his specimen he has nothing but loose and vague terms for it, and fails to hit upon its characteristic features, so that the mycologist cannot recognize the systematic position of the plant from his description ; and on the other hand, the botanist, while careful enough in working out the life-history of his fungus, is too apt, through lack of interest, to go astray in his entomology, and fails to identify his insect, or even sometimes does not so much as determine the order to which it belongs. Such a book as this ought to do much to remedy this defect, showing as it does the imperfection of much existing knowledge, and indicating the gaps which only co-operation can fill up. In the body of the work the claims of the student of insect life seem at first sight to be most prominently recognized. the hosts which suffer from fungoid parasites being treated under their respective orders systematically ; further on is added, for the special benefit of the lover of these lowly plants, a classified list of the fungi, including a mention of their several hosts. To the majority of readers it will no doubt be news that there exists material enough for the compilation of a substantial volume of three hundred and fifty pages on such a subject as this. Few persons except specialists are acquainted with more than two or three kinds of parasitic fungi. There is first the minute form that is so destructive to house-flies, gluing their bodies to the window panes, walls, or chandeliers, while it surrounds the corpses with a sort of whitish halo. Most good museums, again, contain specimens of certain New Zealand or Tasmanian caterpillars from whose bodies project a long horn-like fungus sometimes to the length of several inches. Then there is the species which has earned notoriety by the extent to which it has touched the pockets of a certain section of the commercial world, viz., the celebrated muscardine, which for many years created immense mortality amongst silkworms. But when we have enumerated these we have probably almost reached the limits of popular knowledge ; yet Dr. Cooke enumerates some two hundred species which have already been described as preying upon insects and spiders of different kinds, and this he is able to do without entering at all, except to the extent of the single species that causes the so-called "foul brood" amongst bees, upon the broad field of investigation connected with the minute bacilli and bacteria, which play so important a part in respect of the well-being of both animal and vegetable life. Parasitic fungi attack insects in very different ways. Of course a dead insect will be subject, like all other organic matter, to the growth of moulds, which would probably be of the same species as those that appear on other corpses. But it would be hardly justifiable to call these parasitic, and therefore it is specially with those that attack living insects that our author is concerned. Here the fungus may merely use the insect as a basis of support and be simply attached to its exterior, thus apparently doing little harm ; or it may become a much more insidious enemy by penetrating the body, forming its vegetative portion or *mycelium* at the expense of the tissues of its host, and ultimately bursting through the skin for the formation of its reproductive portion externally. Much depends upon the habits of the insect ; those that live in close or damp situations, beneath the ground, or in burrows in trees, are more liable to attack than those that lead an exposed life. The maturation of the fungus, Dr. Cooke maintains, does not take place till all the interior of the host has been absorbed, and it has therefore succumbed. It is only in this condition, when the spore-bearing receptacles have been developed, that the characteristic features of the fungus are exhibited, and its specific determination becomes possible. Before suffering its fate, the insect, as it finds its end approaching, often climbs into some conspicuous position, clinging with a death-grip to a leaf or stem of grass ; while it remains in this attitude, the fungus passes through its skin, and by its own growing threads attaches the corpse firmly to the substratum on which it rests, while the spores, when developed, find themselves in a suitable position for being conveyed away by the wind to do destruction elsewhere. Dr. Cooke figures various kinds of ants, bees, wasps, beetles, and other insects that have thus become the soil out of which grow branching threads or club-headed columns, by which, in some cases, the appearance of the insect is so greatly modified as to have given rise to the name "Vegetable Wasps." The perusal of this book will serve to impress vividly upon the mind that the dangers to which insects are exposed during their life by no means all arise from the animal world, but that the vegetable kingdom supplies foes quite as insidious and quite as deadly as any that arise from other sources.

Beetles, Butterflies, Moths, and other Insects. By A. W. Kappel and W. E. Kirby (Cassell & Co.).—The authors have catered for beginners in entomology, and for those who, while not professing to be students of the science, yet like to know something about the common insects to which a visit to the country introduces them. They have produced a book of reference which, while it makes no great demands upon either the purse or the time, gives the sort of information such readers require, as to structure, habits, collection and preservation. Between four hundred and five hundred selected representatives of the seven principal orders are briefly described, those, as a rule, being chosen, and wisely so, which are either the commonest or have some special point of interest in their economy. An analytical table for determining the order to which an insect belongs is a useful feature which will be much appreciated by beginners, who are often puzzled as to the nature of their captures. About one hundred and seventy species are figured in twelve coloured plates, which are generally well executed, though with a little tendency to gaudiness, and in our opinion the tinted ground on which they are printed is hardly an improvement.

Letters.

[The Editor does not hold himself responsible for the opinions or statements of correspondents.]

ON THE IMPERFECTION OF OUR KNOWLEDGE OF THE SPECIES OF INSECTS.

To the Editor of Knowledge.

Dear Sir,—Few persons, even among entomologists, have any idea of the vast numbers of insects which exist, or of the immense gaps in our knowledge of even the largest and most conspicuous groups. In the introduction to the new edition of my "Elementary Textbook of Entomology" I have estimated the number of species already described at 270,000, and the actual number of existing species is variously guessed at by different authors at from two to ten millions. Even at the former figure, which I must admit appears to me far too low, it was calculated by Lord Walsingham, in his notable address to the Entomological Society of London, in January, 1890,

that, at the rate of 5800 descriptions of new species per annum, it would require 340 years merely to describe all the undescribed species of insects: and the description of a species is only the beginning of our knowledge of the insect itself in its perfect state. Every species would require many volumes before its life-history, habits, anatomy, physiology, and relations to other species could be dealt with, with anything like completeness. All that any entomologist can accomplish is to add what he can to the gradually accumulating stores of knowledge in the special department of entomology to which he is best able to devote himself by study and inclination.

In England we have about 12,000 species of insects, and it is perhaps not to be expected that the ultimate total, when all the smallest species have been collected and studied as assiduously as the larger ones, will exceed this estimate by more than a few hundred, or at most one or two thousand. But with foreign countries it is very different; and I must confess that I was surprised, when I lately received a fine new species of *Phasmida*, from Madagascar, to find that barely half-a-dozen species had yet been recorded from that island. And yet the *Phasmida*, or stick insects, are among the largest insects. They are generally conspicuous, and easily collected, and Madagascar is an island from which large collections of insects have repeatedly been received of late years; and yet our knowledge of this group of insects as occurring in the island is practically *nil!* If this is the state of our knowledge of such insects as *Phasmida*, how imperfect must it be of the smaller species of *Coleoptera*, *Hymenoptera*, *Diptera*, &c., many of which are of almost microscopic dimensions! Many insects are so local and so closely connected with plants which disappear before civilization, that the same fate of extermination which has fallen on so many of the larger animals during the last century cannot but fall heavily upon these also. It is not too much to say that it is highly probable that a large proportion of the insects at present existing in the world will become extinct before their existence is even known to scientific men.

Yours faithfully,
W. F. Kirby.

THE "WILSON PHENOMENON" OF SOLAR SPOTS.

To the Editor of Knowledge.

Sir,—In his valuable work, "Anleitung zur Durchmusterung des Himmels," published in 1880, Dr. Klein states (page 60) that Wilson was anticipated in his observation of the phenomenon of the varying position of the umbra in the penumbra of a solar spot as it passes across the disc by Maximilian Ludwig Christoph Schülen, of Essingen, in Würtemberg, and this statement is adopted in the last edition of Webb's "Celestial Objects for Common Telescopes."* It is, however, founded on a mistake, the source of which is easy to trace. Wilson was first led to his view that the solar spots were depressions in the photosphere by observations of a great spot which appeared in November, 1769, and he at once communicated his perception of the phenomenon now usually called by his name to the *London Chronicle*. But he did not send his paper on the subject (in which later observations confirming his view are communicated) to the Royal Society until 1773, and it appears in the *Philosophical Transactions* for 1774. No doubt it was by that that foreign astronomers obtained knowledge of his observations. Schülen's (in which he also noticed, he tells us, the funnel-shaped appearance of several spots) were made in the summer of 1771, and communicated first to a newspaper in Stuttgart, the substance afterwards appearing in papers in Switzerland and other places. From that time he heard no more of the matter until, in 1777, he was surprised to see in a French publication the discovery mentioned and ascribed to Wilson.

* In that work Schülen's name appears erroneously as Schüler.

The Rev. F. Wollaston, and also Lalande, expressed their dissent from Wilson's views on the ground that the phenomenon in question is not *always* seen. He replied in the *Philosophical Transactions* for 1783; and surely the objection was insufficient, as some penumbræ might be much shallower than others, too shallow indeed to show the phenomenon in question. It does not appear that he ever heard of Schülen's observations. The latter published at Nördlingen, in 1782, a small treatise under the title "Beiträge zur Dioptrik und Geschichte des Glases," in which he narrates the facts above mentioned respecting his own observations, evidently unaware that Wilson had really anticipated him by more than a year.

As to the phenomenon itself, it is well known that that most persevering and careful sunspot observer, the Rev. F. Howlett, has not in his long course of observations, now extending over thirty-five years, recognized any such funnel-shaped appearance. A letter appears from him on this subject in the number of Knowledge for September, 1889, and it is open to any Fellow of the Royal Astronomical Society to study the long series of sunspot observations presented by him to the Society.

What are we then to conclude? It appears to me that the case is one of those in which a perception given from observations made with small instruments and low powers fails to be seen under the application of larger instruments and higher powers, which furnish clearer and more distinct images of matters of detail; and this is also the view of Dr. Klein.

Schülen's observations, it may be remarked, were made with a non-achromatic telescope of 13½ feet focal length, using a power of about 100. Wilson's telescope was a Gregorian reflector of 26 inches focus, which magnified 112 times. I need hardly refer to the way in which Mr. Howlett's observations have been made, and how it enables his drawings to bring out the minute details of the spots and their surroundings. We must surely concede that the penumbral depression is, in the vast majority of cases, exceedingly shallow.

Yours faithfully,
Blackheath, November 7th, 1892. W. T. Lynn.

THE LIFE-HISTORY OF STARS.

To the Editor of Knowledge.

Sir,—Various theories as to the evolution of stars and their subsequent decadence have appeared from time to time, but they have hitherto been purely theoretical. It seems to me that observation can throw some light on the question; and as far as I have been able to follow its guidance, this light leads in a rather unexpected direction.

A star cooling down will of course fade away gradually, and as the last light which a cooling body emits is red, it was at one time supposed that red stars were in this state of decadence. It seems pretty certain, however, that red stars owe the colour of their light not to their low temperature, but to absorption by an intervening medium.

The best observational test seems to me to be this: If a star has cooled down, and lost a considerable part of its light, it will probably be nearer to us than other stars of the same magnitude; for if its original light was restored, its magnitude would be considerably increased. And if the class of stars which give a particular kind of light are, in the average, nearer to us than stars of the same mag-

nitude which give other kinds of light, this class probably consists of cooled-down stars.

It will be long before our observations on parallax are sufficiently numerous and accurate to enable us to decide this question directly; but there are two other indications of nearness to which we can appeal during the interval. One of these is large proper motion. The other is a computable orbit in the case of a binary star; for I think a pair of stars, separable in the telescope, but revolving so rapidly as to enable an orbit to be computed from the comparatively short range over which our observations extend, must be among our nearer neighbours.

I have tried both these tests and am still making some computations on the subject. The result of both tests is the same. The stars which are nearer to us than other stars of the same magnitude are those whose spectrum is of the type of Capella or Procyon, designated by the letter F in the *Draper Catalogue*.

Comparing the spectrum of a star of this class with that of Aldebaran for instance, we can easily see how the latter might be transformed into the former by the process of cooling. The great number of dark lines in the spectrum of Aldebaran are due to absorption by gases in the atmosphere of the star. As the star cools, many of these gases will condense and the lines due to their absorption will disappear from the spectrum. Partial condensation, again, will render the lines thinner and less dark. These changes will make the spectrum more similar to that of Capella. I do not, indeed, see how a Sirian star could be changed into one of the type of Aldebaran. Mr. Maunder is very probably right in thinking that Sirian and solar stars are physically distinct and do not represent different stages of star-life; but it may, notwithstanding, be true that the Capellan type represents the cooled-down condition of a solar star, and perhaps the higher temperature of the surface of emission in the case of stars of the type of Arcturus or Aldebaran may be indicated by a greater extension of the ultra-violet spectrum. Such an extension undoubtedly exists in the case of Arcturus.

The sun may be regarded as a Capellan star. Mr. Gore not long since maintained in your columns that the sun was a very dull star compared with all others in respect of which we possessed the means of comparison. Should further observations confirm this result, it will strengthen the evidence for the cooled-down condition of the Capellans. With regard to the binary stars to which I have referred the extraordinary brilliancy of γ *Leonis* has been regarded as an exception to the rule as to the greater brilliancy of the Sirians. This star, however, is one of the few binaries with computed orbits whose spectrum is of the Arcturian type, and therefore does not affect my conclusion as to the dulness of the Capellans.

I may perhaps remark that Sirian stars of the class B, which exhibit a number of fine lines in addition to the hydrogen-lines in their spectra, have less proper motion than ordinary Sirian stars. The difference here also may result from condensation.

Should this hypothesis be borne out by further observation the history of the sun may assist in explaining the past geological changes on the earth. There would not be a continuous loss of heat and light in consequence of the cooling. The condensation of vapours in the sun's atmosphere, and consequent diminution in the absorption, might even have the contrary effect; while if they formed into clouds above the photosphere before sinking beneath it we might have a season of great cold.

Truly yours, W. H. S. Monck.

[I do not agree with my friend Mr. Monck that we may assume that the oldest stars are the coolest. The late Mr. Homer Lane, of Washington, showed that a gaseous sphere, losing temperature by radiation and contracting under its own gravity, would actually grow hotter until it ceased to be a "perfect gas" obeying Boyle's law of pressure and density. Mr. Lane's theory is a necessary consequence of the kinetic theory of gases, and those who accept the nebular hypothesis and believe that stars have developed from faintly glowing gaseous masses are forced to assume that during a long period they have grown hotter, and that such stars as are still gaseous are still growing hotter.

We must not be misled by any supposed analogy between the change in tint of a cooling solid and a cooling mass of gas. While a cooling solid passes from white down to a dull cherry-red colour before it ceases to be luminous, some vapours actually give in the electric arc longer lines at the blue end of their spectrum than at the red—that is, the vapour gives out blue light in an outer and cooler region of the electric arc than that in which it gives out red and blue light. But if the stars are like our sun, it is probable that we do not receive the greater part of the light given out by their gaseous masses, which is nearly all intercepted by their photospheres. It is the continuous spectrum of the solar photospheric cloud layer which constitutes the chief light of the sun, and even this reaches us through the solar corona and chromosphere, which subtract many of the wave-lengths.

Therefore, in looking at the light of the stars, if we are to judge by solar analogies, we are observing the continuous spectrum of a comparatively thin layer of incandescent liquid or solid particles, through various depths and various types of stellar coronas and stellar chromospheres. The stellar spectra, when we succeed in fully deciphering and interpreting them, will enable us to determine the qualities and condition of the matter surrounding the stellar photospheres rather than the constituent elements and condition of the matter contained within the stellar photospheres.—A. C. Ranyard.]

STAR-CLUSTER NEAR SIRIUS OBSERVED BY ARISTOTLE.

To the Editor of Knowledge.

Sir,—I cannot find any reference in modern astronomical works to a very ancient observation of the star-cluster near Sirius, which may possibly be of interest to some of your readers. The description is by Aristotle, who has been too much depreciated as a natural historian and philosopher in modern times in consequence of the absurd claim to almost infallibility made on his behalf in the Middle Ages.

The passage to which I refer is in Book I., chapter 6, of his "Meteorologics," where arguing against those who thought that comets were produced by a conjunction of planets, he says that some of the fixed stars have a tail, and that not only the Egyptians assert this, but he can confirm it by his own observation. "For a certain star of those in the thigh of the Dog has a tail, though a dim one; for its light appears feeble to those who fix their gaze upon it, but greater to those who regard it more indirectly."

It is evident that the object thus spoken of is the cluster numbered 41 in the catalogue of Messier, who says that it resembles a nebula under a small power, but appears as a cluster of stars when a higher one is used. In Herschel's "General Catalogue" it is No. 1454; in that of Dr. Dreyer (where there is a misprint in the numerical reference to Messier's list) it is No. 2287. The cluster is readily visible to the naked eye, and is situated about 4° to the south of Sirius.

Yours faithfully,

Blackheath, November 7th, 1892. W. T. Lynn.

THE FACE OF THE SKY FOR DECEMBER.

By Herbert Sadler, F.R.A.S.

During October and the beginning of November, sunspots have been not *quite* so plentiful and large as in the spring. The following are conveniently observable minima of two Algol-type variables:—Algol, December 2nd, 4h. 0m. p.m.; December 17th, 0h. 5m. a.m.; December 19th, 8h. 53m. p.m.; December 22nd, 5h. 42m. p.m. U Cephei, December 4th, 9h. 19m. p.m.; December 9th, 8h. 58m. p.m.; December 14th, 8h. 38m. p.m.; December 19th, 8h. 18m. p.m.; December 24th, 7h. 58m. p.m.; December 29th, 7h. 38m. p.m.

Mercury is an evening star during the first week in December, and a morning one during the last half of it, but owing to his great southern declination he will be liable to be obscured by mists near the horizon. He sets on the 1st at 4h. 52m. p.m., or one hour after sunset, with a southern declination of 24° 52′, and an apparent diameter of $8\frac{1}{4}''$, $\frac{34}{100}$ths of the disc being illuminated. After this he rapidly approaches the Sun, being in inferior conjunction on the 12th. On the 22nd he rises at 6h. 17m. a.m., or 1h. 49m. before the Sun, with a southern declination of 19° 27′, and an apparent diameter of 8·0″, $\frac{3}{10}$ths of the disc being illuminated. On the 27th he rises at 6h. 13m. a.m., or 1h. 55m. before the Sun, with a southern declination of 20° 15′, and an apparent diameter of $7\frac{1}{4}''$, $\frac{5}{10}$ths of the disc being illuminated, and the planet being at his greatest brightness during the month. On the 31st he rises at 6h. 18m. a.m., or 1h. 50m. before the Sun, with a southern declination of 21° 9′, and an apparent diameter of $6\frac{1}{2}''$, $\frac{63}{100}$ths of the disc being illuminated. During the latter half of the month Mercury describes a curved path in Ophiuchus, in a region almost barren of naked eye stars.

Venus is a morning star, rising at 4h. 16m. a.m., or $3\frac{1}{2}$ hours before the Sun, with a southern declination of 11° 17′, and an apparent diameter of $13\frac{1}{2}''$ $\frac{8}{10}$ths of the disc being illuminated, and her brightness being about what it was at the end of last February. On the 16th she rises at 5h. 3m. a.m., or 3 hours before the Sun, with a southern declination of 16° 55′, and an apparent diameter of $12\frac{1}{2}''$, $\frac{83}{100}$ths of the disc being illuminated. On the 31st she rises at 5h. 44m. a.m., or 2h. 24m. before the Sun, with a southern declination of 20° 59′, and an apparent diameter of 12·0″, $\frac{87}{100}$ths of the disc being illuminated, and her brightness being about what it was at the end of January. At 7h. a.m. on the 20th a 9th magnitude star will be 26″ north of the planet. During the month Venus passes from Virgo through Libra into Scorpio. When rising on the 23rd she is about 36′ *nf* the 3rd magnitude star β Scorpii, and at 6h. a.m. on Christmas Day she is $\frac{3}{4}$ of a minute of time preceding and 6′ south of ν Scorpii, $4\frac{1}{4}$ magnitude, β being a pretty double star, and ν being a triple in moderate-sized telescopes; both of the large stars composing β and ν being again double in large instruments.

Mars is an evening star, but is rapidly getting fainter and smaller, though he is in a much better position as regards elevation above the horizon. He sets at 11h. 38m. p.m. on the 1st, with a southern declination of 7° 30′, and an apparent diameter of 9·5″, the defect of illumination on the following limb being obvious. His brightness is then only one-eighth of what it was at opposition. On the 16th he sets at 11h. 38m. p.m., with a southern declination of 3° 23′, and an apparent diameter of 8·5″. On the 31st he sets at 11h. 36m. p.m., with a northern declination of 0° 45′, and an apparent diameter of 7·6″, his brightness being then only one-thirteenth of what it was at opposition. He is in quadrature with the Sun at midnight on the 9th. During December Mars describes a direct path from Aquarius into Pisces. At 8h. 30m. p.m. on the 2nd a $9\frac{1}{2}$ magnitude star will be 20″ north of the planet. Early on the evening of the 5th Mars will be 12′ south of the 4th magnitude star ϕ Aquarii, and at about 9 p.m. on the 7th he will be $7\frac{1}{2}'$ north of the $5\frac{1}{2}$ magnitude star 96 Aquarii.

Ceres is an evening star, southing on the 14th at 9h. 35m. p.m., with a northern declination of 9° 35′. At the end of the month she is as bright as an 8th magnitude star. During December she pursues a short retrograde path from Taurus into the borders of Aries. At about 7h. 30m. p.m. on the 4th she is 20′ south of the $6\frac{1}{4}$ magnitude star Weisse's Bessel², iiih. 275, and about 7 p.m. on the 26th she is $7\frac{1}{2}'$ north of and a little following the $6\frac{1}{4}$ magnitude star B. A. C. 987. Vesta is also an evening star, southing on the 14th at 9h. 19m. p.m., with a northern declination of 8° 48′. At the end of the month she is as bright as a $7\frac{1}{3}$ magnitude star. During December she pursues a retrograde path in Cetus, being 24′ north of the 5th magnitude star λ Ceti at 9h. p.m. on the 16th.

Jupiter is an evening star, still holding his position as the most magnificent object in the evening sky. On the 1st he rises at 1h. 47m. p.m., with a northern declination of 4° 37′, and an apparent equatorial diameter of 45·1″, the phase on the following limb being now very apparent. On the 16th he rises at 0h. 47m. p.m., with a northern declination of 4° 54′, and an apparent equatorial diameter of 43·0″, the phase amounting to 0·4″. On the 31st he rises at 11h. 48m. a.m., and souths at 6h. 18m. p.m., with a northern declination of 5° 1′, and an apparent equatorial diameter of 40·9″. During the greater part of the month he describes a short direct path in Pisces. At 2 p.m. on the afternoon of the 27th he is in conjunction with the Moon, $31\frac{1}{2}'$ to the north of her, and the same evening at 9 p.m. he is in conjunction with the 6th magnitude star 73 Piscium, Jupiter being $10\frac{1}{4}'$ south. The following phenomena of the satellites occur while Jupiter is more than 8° above and the Sun 8° below the horizon. On the 1st an occultation reappearance of the third satellite at 5h. 45m. p.m.; an eclipse disappearance of the same satellite at 8h. 5m. 52s. p.m.; and an eclipse reappearance at 10h. 1m. 59s. p.m. On the 3rd an occultation disappearance of the first satellite at 9h. 54m. p.m. On the 4th a transit ingress of the first satellite at 7h. 3m. p.m.; of its shadow at 8h. 12m. p.m.; a transit egress of the first satellite at 9h. 17m. p.m.; an occultation disappearance of the second satellite at 10h. 11m. p.m.; and a transit egress of the shadow of the first satellite at 10h. 26m. p.m. On the 5th an eclipse reappearance of the first satellite at 7h. 41m. 49s. p.m. On the 6th a transit egress of the shadow of the first satellite at 4h. 55m. p.m.; a transit ingress of the second satellite at 5h. 8m. p.m.; a transit ingress of its shadow at 7h. 30m. p.m.; a transit egress of the satellite at 7h. 41m. p.m., and of its shadow at 9h. 58m. p.m. On the 8th an occultation disappearance of the third satellite at 7h. 5m. p.m., and its reappearance from occultation at 9h. 27m. p.m. On the 10th an occultation disappearance of the first satellite at 11h. 44m. p.m. On the 11th a transit ingress of the first satellite at 8h. 54m. p.m.; a transit ingress of its shadow at 10h. 8m. p.m., and a transit egress of the satellite one hour later. On the 12th an occultation disappearance of the first satellite at 6h. 12m. p.m., and its reappearance from eclipse at 9h. 37m. 39s. p.m. On the 13th a transit egress of the first satellite at 5h. 36m. p.m.; of its shadow at 6h. 50m. p.m.; a transit ingress of the second satellite at 7h. 37m. p.m.; of its shadow at 10h. 7m. p.m.; and a transit egress of the satellite itself three minutes later. On the 15th an eclipse

reappearance of the second satellite at 6h. 49m. 12s. p.m.; and an occultation disappearance of the third satellite at 10h. 48m. p.m. On the 18th a transit ingress of the first satellite at 10h. 45m. p.m. On the 19th a transit ingress of the shadow of the third satellite at 6h. 12m. p.m.; an occultation disappearance of the first satellite at 8h. 5m. p.m.; a transit egress of the shadow of the third satellite at 8h. 19m. p.m., and an eclipse reappearance of the first satellite at 11h. 33m. 31s. p.m. On the 20th a transit ingress of the first satellite at 5h. 14m. p.m.; a transit ingress of its shadow at 6h. 32m. p.m.; a transit egress of the satellite at 7h. 28m. p.m., and of its shadow at 8h. 45m. p.m.; a transit ingress of the second satellite at 10h. 9m. p.m. On the 21st an eclipse reappearance of the first satellite at 6h. 2m. 33s. p.m. On the 22nd an occultation reappearance of the second satellite at 6h. 56m. p.m.; an eclipse disappearance of the satellite at 7h. 4m. 13s. p.m., and its reappearance at 9h. 25m. 41s. p.m. On the 26th a transit egress of the third satellite at 7h. 9m. p.m.; an occultation disappearance of the first satellite at 9h. 58m. p.m., and a transit ingress of the shadow of the third satellite at 10h. 15h. p.m. On the 27th a transit ingress of the first satellite at 7h. 7m. p.m.; a transit ingress of its shadow at 8h. 28m. p.m.; a transit egress of the satellite at 9h. 21m. p.m., and of its shadow at 10h. 41m. p.m. On the 28th an eclipse reappearance of the first satellite at 7h. 58m. 24s. p.m. On the 29th a transit egress of the shadow of the first satellite at 5h. 10m. p.m.; an occultation disappearance of the second satellite at 6h. 55m. p.m.; an occultation reappearance of the same satellite at 9h. 29m. p.m., and its eclipse disappearance at 9h. 41m. 11s. p.m. On the 31st a transit egress of the shadow of the second satellite at 7h. 13m. p.m.

Saturn does not rise till after midnight on the last day of December, and we therefore defer giving an ephemeris of him until next year. He is occulted by the Moon, though the phenomenon is not of course visible in the British Islands, on the evening of the 12th. Uranus does not rise before 3 a.m. at the end of the month, and is therefore, for the purposes of the amateur observer, invisible.

Neptune is an evening star, and is excellently situated for observation, coming into opposition with the Sun on the 1st, at a distance from the earth of about 2,680,700,000 miles. He rises on the 1st at 3h. 54m. p.m., with a northern declination of 20° 22′, and an apparent diameter of 2·7″; on the last day of the month at 1h. 53m. p.m., with a northern declination of 20° 15′. During December he describes a short retrograde arc in Taurus, between τ and ϵ, to the N.E. and N.W. of the $5\frac{3}{4}$ magnitude star Weisse's Bessel[2], ivh. 650. At 11h. 40m. p.m. on the 2nd he will be 1′ 0″ due north of a 9th magnitude star. A map of the small stars near his path will be found in the *English Mechanic* for October 28th.

December is a fairly favourable month for shooting stars, the chief showers being those of the Geminids on December 9th—12th, the radiant point being in r.a. 7h. 0m., and north declination 32°, rising about 4h. 10m. p.m., and setting at 1h. 40m. a.m., and of the Andromedes, occurring on the evenings of the 26th and 27th, the radiant point being in r.a. 1h. 40m. and north declination 43°, the shower being circumpolar. It seems probable that a fine display will occur this year.

The Moon is full at 2h. 17m. a.m. on the 4th; enters her last quarter at 2h. 30m. a.m. on the 11th; is new at 8h. 13m. a.m. on the 19th; and enters her last quarter at 9h. 22m. p.m. on the 26th. She is in perigee at 3·0h. a.m. on December 2nd (distance from the earth 224,630 miles); in apogee at 1·3h. p.m. on the 15th (distance from the earth 252 240 miles); and in perigee at 0·2h. p.m. on the 31st (distance from the earth 222,320 miles). She is at her greatest western libration at 5h. 48m. a.m. on the 9th, and her greatest eastern at 5h. 21m. p.m. on the 24th.

Chess Column.

By C. D. Locock, B.A.Oxon.

All communications for this column should be addressed to the "Chess Editor, *Knowledge Office*," and posted before the 10th of each month.

Solution of November Problem:—

Key-move: 1. K to Kt8.

If 1. . . . K to Q5, 2. R to Q2ch, etc.

If 1. . . . P to Q5, 2. Q to Kt7, etc.

If 1. . . . K to Q6, 2. Q to KB4, etc.

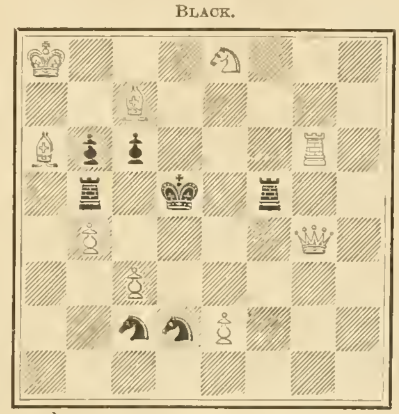
Correct Solutions received from Alpha, A. Rutherford, and H. S. Brandreth.

PROBLEM.

By E. Halliwell, Bolton.

First Prize in *Bristol Mercury* Tourney.

Black.



White.

White to play, and mate in two moves.

The subjoined game resulted in the first defeat sustained by Mr. Lasker at the Manhattan Club, New York. The winner is well known as one of the strongest American players.

Ruy Lopez.

White (E. Lasker).	Black (A. B. Hodges).
1. P to K4	1. P to K4
2. KKt to B3	2. QKt to B3
3. B to Kt5	3. P to Q3
4. QKt to B3 (*a*)	4. B to Q2
5. Castles	5. KKt to K2 (*b*)
6. P to Q3 (*c*)	6. Kt to Kt3
7. B to K3 (*d*)	7. B to K2
8. P to Q4	8. Castles (*e*)
9. B to B4	9. B to Kt5?
10. P to Q5 (*f*)	10. Kt to Ktsq
11. P to KR3	11. B × Kt (*g*)
12. Q × B	12. P to KB4
13. P × P	13. Kt to R5
14. Q to Kt4 (*h*)	14. Kt × BP
15. B to Q2	15. Kt to Q2
16. B to Q3	16. P to KKt3
17. Kt to K4	17. R to B2
18. QR to Ksq (*i*)	18. Q to KBsq
19. Kt to B3	19. P to QR3
20. Kt to Qsq	20. Q to Kt2

21. B to B3	21. QR to KBsq
22. P to Kt4 (*j*)	22. B to Qsq
23. B to Kt2	23. Kt to B3
24. Q to QB4	24. Kt to R4 (*k*)
25. P to KB4	25. P to QKt4
26. Q to B6	26. Kt to K2 !
27. Q × RP	27. Kt × BP
28. Q × KtP	28. Q to R3
29. Kt to B2 (*l*)	29. Q to Kt4
30. B to K4	30. Kt to KB4
31. Q to B4 (*m*)	31. Kt to Kt6
32. KB to B3	32. Kt × R
33. R × Kt	33. Q to R5 !
34. Q to K4 (*n*)	34. Kt × Pch !
35. Kt × Kt	35. Q × Q
36. B × Q (*o*)	36. R × Rch
37. K to R2	37. R to K8
38. B to Q3	38. P to K5
39. B to B4	39. B to B3
40. B × B	40. R × B
41. K to Kt3	41. P to K6
42. Kt to Kt5	42. R to B7
43. B to Q3	43. R to KKt8
44. Resigns.	

NOTES.

(*a*) New, but unenterprising. 4. B × Ktch and 4. P to Q4 are both stronger.

(*b*) A very good development in this position. Black will evidently have the first opportunity for advancing his P to KB4.

(*c*) The Pawn should go two squares; *vide* his 8th move. 6. B to QB4 (threatening Kt to KKt5) would be met by Kt to R4 or Kt to Kt3.

(*d*) Mr. Lasker inherits Paul Morphy's predilection for this move. In the present case the Bishop seems well posted, being unassailable by either of the adverse Knights.

(*e*) Black does well in refusing to exchange. Taking the Pawn would give freedom, by slaughter or otherwise, to White's useless King's Knight. At his next move, however, he should play K to Rsq, with a view to the advance of the KBP.

(*f*) Seldom advisable, the present instance being no exception. He might play with safety 10. P × P, QKt × P; 11. B to K2.

(*g*) The Bishop is too valuable to exchange. We should much prefer B to Bsq, to be followed by working the QKt round to KB5 if possible.

(*h*) The Queen is liable to be attacked here later on. Probably 14. Q to K2 would be a safer line of play.

(*i*) This results in overcrowding. There seems no objection to the obvious Kt to Kt5. White's play now becomes unintelligible for a time.

(*j*) This advance of the unsupported QKtP is a favourite manœuvre of Mr. Lasker's. On principle it is clearly weakening for the end-game.

(*k*) Prettily played. Clearly White cannot play 25. P to KKt4, Kt to B5; 26. P × Kt? Nor would he gain anything by 26. Q × Kt, P × Q; 27. B × Q, Kt × B, etc. After this Black gives up a Pawn in order to secure the KBP with safety, and at the same time leave the White Queen out of play.

(*l*) He is compelled to guard the RP which will be threatened next move; but there was another way by K to R2, which was possibly better.

(*m*) The Queen continues to return by easy stages. He might have saved the exchange among other ways by 31. B to KB3.

(*n*) The Queen is obviously threatened. 34. K to R2 is useless on account of 34. . . . Kt × KtP ! The same objection applies to 34. Q to B3. The game in fact cannot be saved, and Mr. Hodges proceeds to bring the game to a clever and rapid termination.

(*o*) A process in which he is not assisted by Mr. Lasker, who overlooked the best reply at this point, and discovered it eight moves later.

CHESS INTELLIGENCE.

In the Handicap Tournament of the British Chess Club, Mr. Trenchard has come out the winner of one section: in the other Mr. Donisthorpe has the best chance. The winner of this section will play Mr. Trenchard for the first prize.

Mr. Lasker has been playing the leading New York players with almost uniform success. It is hoped that a match with S. Lipschütz will be arranged.

January 28th is the date fixed for the North *v.* South match at Birmingham. There will be 100 players on each side, with ten reserves. Mr. Blackburne has accepted the post of umpire, no sinecure in a match arranged on such a gigantic scale, and with the time of play probably limited to about four hours.

A match by correspondence is in progress between the Berlin and Leipsic Clubs. The former are represented by Messrs. Alapin, Hirschfeld, Von Scheve, Walbrodt and Winawer: while the Leipsic committee consists of Messrs. Bardeleben, Max Lange and Mieses. Two games are being played, the time limit averaging five days a move.

A match was played recently at Newcastle between Messrs. Bird and Heywood, Chess Editor of the *Newcastle Chronicle*. Three games were played on even terms, of which Mr. Bird won two and lost one. Three games at the odds of Pawn and move resulted in equality. Three other games, in which Mr. Bird conceded the large odds of Pawn and two moves, resulted in one win to Mr. Heywood and one drawn game: the final game was not played, Mr. Bird resigning the game and the match.

A second edition of the "Chess Openings" by Messrs. Freeborough and Ranken is in the press. The price will be 6s. 6d. net.

CONTENTS OF No. 85.

TERMS OF SUBSCRIPTION.

"KNOWLEDGE" as a Monthly Magazine cannot be registered as a Newspaper for transmission abroad. The Terms of Subscription per annum are therefore as follows:—To any address in the United Kingdom, the Continent, Canada, United States, Egypt, India, and other places in the Postal Union, the Subscription is 6 shillings, including postage; or 1 dollar 50 cents; or 6 marks; or 7 francs 50 cents.
For all places outside the Postal Union, 5 shillings in addition to the postage.

Communications for the Editor and Books for Review should be addressed Editor, "KNOWLEDGE" Office, 326, High Holborn, W.C.